

Psychological Review

THEODORE M. NEWCOMB, Editor
University of Michigan

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THE PSYCHOLOGICAL REVIEW

EFFECTS OF THE GESTALT REVOLUTION: THE CORNELL SYMPOSIUM ON PERCEPTION¹

JULIAN E. HOCHBERG

Cornell University

"Structuralism" and the Gestalt revolution. Before the turn of the century, the psychology of sensation and perception enjoyed in "structuralism" a relatively unified approach. New data and interests soon forced the system apart at the seams; as to perception, the most important criticisms came from the Gestaltists. One main purpose of this symposium was to consider what areas of agreement could be found three decades after the introduction of *Gestalttheorie*. Discussion centered around two areas of research: perceptual change

(or learning), and the perception of "events." Let us review briefly what made these areas important to Gestalt theory.

The "Constancy Hypothesis" and "Event Perception." Structuralism tended to assume that: "... things look as they do because the proximal stimuli are what they are" (16, p. 80), and that "... the result of a local stimulation is constant ... that all locally stimulated excitations run their course without regard to other excitations" (16, pp. 96 f.)—the "Constancy Hypothesis." Any failure of this relationship was explained in terms of previous associations: "... things do not look as they ought to on the ground of pure perceived stimulation, and they differ from such an expectation by looking more like ... the things with which we have real dealings. ... in dealing with things we acquire experience about them, and this experience enters our whole perception" (16, pp. 84 f.).

It is easy to embarrass the Constancy Hypothesis. We can obtain the same responses to different stimuli or different responses to the same stimuli, as in the phenomena of object constancy, the geometrical illusions, etc. (16). Auditory stimuli affect visual experience, and vice versa (24); forms appear identical when stimulating to-

¹ Participants were: Egon Brunswik, University of California; James Drever, University of Edinburgh; James J. Gibson, Cornell University; Fritz Heider, University of Kansas; Julian Hochberg, Cornell University; Gunnar Johansson, University of Stockholm; George Klein, New York University; Ivo Kohler, University of Innsbruck; Robert B. MacLeod, Cornell University; Wolfgang Metzger, University of Munster; T. A. Ryan, Cornell University; Hans Wallach, Swarthmore College.

This symposium, held in June, 1954, was made possible by a National Science Foundation Grant. No attempt was made to preserve either the specific order of discussion, or the identity of the source of each opinion, though I have in a few instances inserted names of contributors. Since such reworking tends to take the responsibility out of the hands of the other participants, I have endeavored to return it to them by circulating copies of the report for their comments, addenda, and post-mortem alterations, and to take account of their replies, where length permitted.

tally diverse elements,² yet are rendered unrecognizable without changing the elements (16). The Gestaltists held that the theoretical error lay in the choice of too-small units of analysis; this was to be avoided by a "naive" phenomenology as opposed to formalized analytic introspection. Moreover, the "elements" are not independent, and the "laws" of their interaction were to be understood in terms of a new and unorthodox view of the underlying molar cerebral processes.

The phi phenomenon is a good example here. The (illusory) apparent motion can be made as completely convincing as real motion; the explanation of perceived real motion as the tying together by association of successive sensations of position becomes questionable. The apparent motion is a unitary experience, and Wertheimer started the Gestalt search for "brain models" by postulating an equally unitary cortical process. Perhaps more important, when Korte (18) investigated the dependence of the phi phenomenon upon brightness, separation, etc., a new form of psychophysics began to emerge, in which the experience was a full event, rather than a fragmentary "sensation," and in which *the stimulus variables manipulated were not necessarily ones which seemed intuitively similar to, or attributively responsible for, the quality of the response*. In general, Gestaltists hoped to restore stimulus-response correlations by treating entire configurations as the stimuli, and entire phenomenal events as the responses; in the process, the simple associational formulae of perceptual learning were discarded. Note that the Gestalt theorists

were *not* nativists in the traditional sense. They did not deny that learning affects perception. They denied only that all departures from the Constancy Hypothesis are to be explained in terms of past associations.

ADAPTATION TO THE DISTURBANCE OF STIMULATION: PERCEPTUAL RELEARNING

Up-Down Inversion

The effects of natural visual disturbances (long-term cataracts, etc.) have been reviewed (11), and contribute to a modern version of the old nativist-empiricist controversy; despite the recognized importance of the Stratton experiment (27), however, few experiments have been performed with humans (6, 27). In Erismann's laboratory, different kinds of protracted disturbance are under investigation, and Ivo Kohler's (cf. 17) findings are striking and important.

Let us survey the course of adaptation to visual disturbance, especially in inversion (in which up becomes down and vice versa).

Stage 1. When disturbing spectacles are first put on, the world seems strange in various ways: faces look unfamiliar, walking people seem mechanical (the up-and-down component, normally not "seen," becomes apparent), brightness contrast seems greater, colors more saturated. As head and eyes move, the normally stationary world swings about. The unfamiliarity of faces eventually wears off; in later stages, one can recognize features, etc., but even to the last, expressions cannot be discerned. The subject is almost incapacitated, with his motor actions appropriate at best to a world which appears upside down.

Stage 2. The subject can negotiate streets, can "fence" with the experimenter, etc. However, we cannot accept such task performance as the op-

²A recent experiment by Wallach and Austin (2) makes it necessary to qualify this statement, however, since it now appears that there is at least some dependence of form recognition on retinal locus of original presentation.

erational index of perception, or perceptual response (cf. 23), without residuum: *despite such effective motor performance, the world remains phenomenally inverted!* Perceptuo-motor adjustment, and phenomenal adaptation, run separate (but related) courses, and we cannot safely equate the two. Phenomenal invertedness first disappears for objects connected with the subject's body-system, or with clear indications of gravity. An object grasped, a plumb line, a face with a cigarette between its lips, with smoke ascending from the tip—these are seen as upright for a short time.

The relationship between the phenomenal upright, obtained through "adaptation" to an inverted stimulus, and the original phenomenal upright is by no means clear. Kohler reported that if a subject is shown two faces, one upside down (so that its retinal image is now, through the inverting spectacle, that which would without the spectacles have occurred from a face right side up) alongside an upright (and hence retinally inverted) face with a smoking cigarette between the lips, now *both* appear "upright"—*but in different ways and in opposite directions!* Kohler plans to adapt only *one* eye to inverting prisms, to permit further investigation of this question.

Stage 3. About a month later, the perceived world is almost continuously congruent with geography and independent head movements, even while the subject performs complicated behavior (bicycling, etc.). A specific familiar object (e.g., a given house) will appear unfamiliar when first viewed through the spectacles, even after other objects (e.g., other houses) have achieved familiarity through adaptation (*Stage 3*). Objects familiarized only through the spectacles become unfamiliar when the spectacles are removed. Transfer is restricted both for visual and motor

adaptation, and the process is not in any sense a learning to "invert the whole stimulus field."

Stage 4. If the spectacles are now removed, *the world appears inverted with normal stimulation.*

Form Distortions

Adaptation to visual inversion may *not* be generalized, but adaptation to other disturbances may be. Wedge-deforming prisms contract one side of the field horizontally and expand it vertically, as compared with the other; phenomenal adaptation eventually occurs and, with spectacles removed, the reverse form distortion appears as an after-effect. Two classes of distortion effects appear: (a) general throughout the field, such as the *curvature* of lines; (b) conditional upon direction of gaze, such as the *angles* at which lines join.

Adaptation to width distortions (measured by rotation of an additional prism to eliminate phenomenal distortion) was slowest and weakest. Then came adaptation for curves, then for angles (at the right stage a subject will correctly see a square as *right-angled*, *but with curved sides*—an example of the inconsistency apparently permissible in the docile geometry of the visual field),³ with adaptation to distortions of movement fastest and strongest.

Tentatively, therefore, it appears that adaptation may be faster when apparent differences between disturbed and familiar conditions are greater. Thus, adaptation is faster to up-down inversion than to right-left reversal, perhaps because our environment shows more bilateral symmetry around a vertical than a horizontal axis: gravity is a

³ However, we cannot be sure from Kohler's report that this necessarily involves any visual inconsistency; if, as was not ascertained, the subject sees the square as being on an appropriately curved surface (Ryan), no inconsistency remains.

universal, whereas there are few comparable universals in a left-right sense.

*Adaptation to Color-Fringes
(Half Spectra)*

When viewed through prisms, each achromatic contour bears a fringe of color—a red-yellow half-spectrum to the left, blue to the right. This effect, also, is *general*, since the fringes appear everywhere in the field, independent of eye position. It is also *differential* in that, for any contour, the fringe is red-yellow on one side, and blue on the other.

After about a month, adaptation is complete: no more color-fringes are seen! *When the spectacles are removed, every contour in the field now appears surrounded by a reversed color fringe, i.e., blue on the side to which the red-yellow previously appeared, and vice versa.* If each eye adapts separately to opposite prismatic color fringes, each eye displays (in rivalry) its own after-effect. These data appear innocent enough, but are actually quite resistant to a satisfactory yet detailed explanation.

Split-Field Chromatic Disturbances

The differential effects were more strikingly isolated as follows. With the left half of each spectacle lens blue and the right half yellow, white objects at first appear blue when viewed to the left, and yellow when viewed to the right. The usual desaturation appears if the eyes are fixed to one side but, obviously, as soon as they move to the other side, the complementary color appears. The astonishing fact is that, after protracted adaptation, eventual disappearance of all chroma occurs despite eye movement, in which the light at the fovea is at one time blue, at the next, yellow, and color is seen in neither case. With spectacles removed, *the aftereffect is also conditional upon eye*

position: with the eyes left, the world appears yellow; with the eyes right, blue! Thus, both effects are tied to, and therefore phenomenally independent of, eye movement.

*Discussion of the Adaptation to
Disturbed Stimulation*

Traditionally, for each sensation one could isolate one stimulus, and vice versa; perceptions entailed past associations, "unnoticed sensations," and "unconscious inference." The study of perception was possible only because, despite such associations, the underlying sensations remained tied to the stimulus and were rigorously investigable by the psychophysical methods. After Gestalt (and other) emphasis on the indistinguishability of sensation and perception, at least three extreme alternatives remained open:

1. To reconnect stimulus and percept by discovering the physiological mechanisms responsible for the (presumably) "organizational" lack of correspondence (cf. 16, 30); this has not, thus far, proved very fruitful.
2. To re-analyze the stimulus field in order to determine what aspects or "higher order variables," if any, *are* in correspondence with perceptual response (cf. Gibson, 9). This treats *perception* psychophysically as previously only *sensation* was investigated, and seeks to restore a new version of the Constancy Hypothesis.
3. To treat all stimulus-response relations as previously *perception* was investigated, i.e., as so determined by the organism's past experience with an unreliable environment that only the loosest, most statistical connection between proximal stimulus and perceptual response can be expected. Kohler's experiments constitute the greatest systematic change of relationship between stimulus and experience as yet reported,

and are well suited for examination of this issue.

Brunswik's approach comes closest to the third position: the correspondence between the response and the physical (distal) stimulus must be imperfect. This ambiguity, supposedly inherent in the equivocal relationship between distal stimuli and the proximal stimuli (the *cues*) to which the organism can respond, means that each cue has only a limited probability of being "correct." Thus, an *ecological sampling* reveals that the correlation is less than 0.6 between the cue of vertical position and the distal stimulus distance, and less than 0.4 between the cue of "space filling" (number of distinguishable intervening steps) and actual distance. An adjusted organism therefore must use many cues, weighting each according to its relative frequency (ecological validity) by some process of probability learning, of which Brunswik postulates two varieties: (a) *distribution learning*, and (b) *correlation learning*. Distribution learning (e.g., that hanging objects are rare) is very general, and, with enough reversals of cases, a "revolution" in such expectancies will occur. Correlation learning grasps concomitance, the reliability of any cue as a correlate of the object. Perception is, to Brunswik, a reasoning-like ("ratio-morphic") process, much faster to respond than is reasoning, but slower to change what has been learned; it is a speeded up, conservative, stereotyping, reasoning-like function (3).

The distributional perceptual learning provisions which are at the heart of this viewpoint may *in principle* accommodate Kohler's general findings, but they do not begin to explain the very singular specific data reported. Despite the apparent ease of the first Gestalt attack on the empiricist position, it is extremely difficult to refute any general

"explanation" couched in terms of past experience. Consequently, we must *specify* the relationship between past and present stimuli and percepts, or we have "explained" little more by invoking "past experience" than by invoking "human nature." A more specific attempt to account for the conditional effects through "conditioning" to the eye movements—or, more accurately, through extinction of previously conditioned responses (Drever),—did not seem very fruitful, although it may be possible to do more justice to findings with the greater number of assignable variables in the Hullian repertoire.

Metzger proposed as a Gestalt theoretical explanatory principle that *aspects of the visual world tend to become independent of behavior*, classing Kohler's results with the various perceptual constancies of size, motion, etc. (and explaining why reversed binocular disparity never adapted, since no added stability re the individual's behavior would thus be gained). However, this fails to explain adaptation to the half-spectra, in which the viewer's activity is hard to denote, and these phenomena seem at once the most general and the most puzzling. This proposal also leaves unexplained the fact that many aspects of the disturbance fail to reach complete adaptation, e.g., curvature adaptation appears asymptotic at about one-third. Metzger suggested that the "stresses" toward straightening are not strong enough to overcome completely the curvature of the actual stimulus; however, as Wallach pointed out, if the "internal stresses" of "organization" are insufficient to straighten an extreme curvature, adaptation should go farther with weaker prisms; yet Kohler reported that after prisms of a given strength caused an asymptotic degree of distortion-adaptation, prisms of double that strength cause adaptation to start

again, with a new asymptote, still short of complete correction.

Why not, Gibson asked, from a position he had previously taken on the problem of curved line adaptation (8), suppose that *the physical norm tends to become the phenomenal neutral point*? Such norms must still be defined,⁴ but the consensus appeared to be, at this point, that a process of "partialing out" norms does seem to be a general principle during adaptation: what is *always* present becomes "unnecessary" to perceive. The blue half-spectrum to the right is always present, and so becomes the chromatic norm, and white light, the previous norm, will appear yellow. The problems affecting the previous formulations seem less troublesome from this point of view, but before its utility can be evaluated, much more explication is necessary.

Before we essay any conclusions, however, let us turn to the second general problem, that of *event perception*.

EVENT PERCEPTION

Recent advances have been made in three divergent salients—events involving the motion of several parts (Johansson, 15; Michotte, 22; Duncker, 5); the perception of social events (Heider and Simmel, 12); the kinetic depth effect, i.e., the depth perception consequent to sequential proximal stimula-

tion from a rotating distal stimulus (Wallach and O'Connell, 29; Metzger, 20).

The General Problem

We know that one can see motion when there is none on the retina. Johansson's series of studies (15) permit some fairly definite psychophysical relationships to be laid down for the perception of motion.

A horizontal row of four lights simultaneously moving up and down with the same phase appears as one entity in motion. With two pairs of lights 180° out of phase, "common fate" acts as a first, primitive law: those moving at the same time in the same direction appear as parts of one object. With more complex phase relations, those elements with the least phase difference form a group. Three-dimensional arrangements appear if they preserve a constant spatial distance between the elements in relative motion, picking out a constant shape in motion (cf. Metzger, 20, 21). When we depart from parallel harmonic motions to such motions at angles to each other ("Lissajous combinations"), the situation changes. With two moving lights having perpendicular paths, the same frequency, and phases such that they come to the point common to the two paths at the same time, they appear to move toward and away from each other *along the sloping line joining their extreme positions*. In addition, there appears a slight motion of the whole system in the perpendicular direction. If two points move in opposite directions with the same frequency in circular paths which, touching at one point, approach each other, they fuse, and then retreat on a horizontal line; simultaneously, the path along which they travel moves up and down. In short, *the physical motion which is present in the stimulus appears to have disappeared without any trace in per-*

⁴Such norms need not be defined simply by statistical analysis of the distal stimulus. The straight line among curved lines, the rectangle among acute and obtuse angles, etc., are norms in other than a frequency sense: they are neutral points in "opposition series" (cf. 8), which run, say, from concave to the left to concave to the right, from obtuse to acute, etc. These are norms in a logical rather than an actuarial sense; a mathematical analysis of the visual world around us will be simpler, as Kohler puts it, if our reference tools are straight lines and right angles than if they are curves or obtuse or acute angles, so that the straight line and right angle would be analytic norms even if they never occurred in experience.

ception, while a movement is perceived which appears to have no simple counterpart in the stimulus. However, the perceived motions *do* correspond to component vectors into which the stimulus motions may be analyzed: "... the phenomenon can fittingly be described as motions relative to two different co-ordinate systems, one fixed, and the other moving, in accordance with the principles for relative motion" (15, p. 97).

Thus, one can "extract" a given component from a complex motion and obtain the predicted "remainder." For example, a simple straight-line harmonic motion may be analyzed into two harmonic motions of lower amplitude and a 45° phase difference. Consequently, if one point is given an harmonic motion, and a second point is given the motion of one of its two components, the relative motion should be that of the remaining component; and in fact this is what is seen. One point moves back and forth, and the other moves back and forth with respect to it, like a planet rotating around its sun (in fact, although this *can* be seen as motion in a flat plane, many subjects see it in three dimensions, as an orbital motion).

In general, a "figural hierarchy" of motion is perceived: first, a static background; next, and in reference to this, the common motion; and highest, the components of motion relative to the common motion. If we have one point *a* moving in a circular path, and the other *b* in a vertical path, point *a* appears to move toward and away from *b* in a horizontal path, while both *a* and *b* move up and down. With *b* removed, *a* is seen to move in a circle; with *a* removed, *b* is seen to move vertically. If we repeat the procedure, but with the vertical path of *b* shorter than the diameter of *a*'s circular path, both points *a* and *b* move vertically together, with *a* describing an ellipse, and the vertical

component of *a*'s motion has lost the length of travel "used up" by *b*'s motion. In general, of two motions which are the same in other respects, the motion which has the shorter path or, more generally, the *lower velocity*, will determine the magnitude of motion of the combined system. It is this combined motion which forms the frame of reference within which the remaining motions occur. If two or more simultaneous motions or components of motion, equal in magnitude and direction, are analyzable within the stimulus motion, these will be seen as a single motion.

In general, Johansson states his findings as follows: In every case, if we abstract the motion components common to all of the moving points (as a special case, the common motion may reduce to zero), the remaining components become the relative motion of the parts, while the common motion becomes the motion of the whole relative to the stationary background. It is therefore always the shortest excursion which will determine the common motion.

Motion and Three-Dimensional Space: The Kinetic Depth Effect

In many of Johansson's experiments, the percept was three-dimensional, even though both proximal and distal stimuli were two-dimensional.

Just as we can study the psychophysical relationships in apparent motion without *necessary* concern with ontogenetic or historical antecedents, so can the emergence of depth as an aspect of event perception be studied in its own right. In fact, specific knowledge concerning perceptual learning begins to emerge from such study, to replace gross appeals to empiricism.

Particularly informative here are the investigations of the "kinetic depth effect" (29). Wallach was originally concerned with depth perception without

the "primary" depth cues (binocular disparity, etc.). In an at least partially empiristic view, one originally had "depth" experience only where there were such "cues" (making the case of monocular individuals awkward to explain); in terms of the "spontaneous organization" of *Gestalt-theorie*, we are just lucky, phylogenetically speaking. However, as one walks around in the world of objects, the proximal stimuli projected by distal objects are pretty much those which would be cast by the axial rotation of those objects, and the rotation of most of the objects around us will yield three-dimensional experience even when viewed monocularly. Can subjects (monocular or otherwise) "learn" from this kinetic effect, while in motion, the three-dimensional nature of a given object? (Remember also the importance of motion in the "relearning" of space in Kohler's findings.)

Wire patterns were chosen whose projections evoked two-dimensional responses; these gave way to three-dimensional responses when the wire forms were rotated. After this experience with a given form, it would tend in the future to appear three-dimensional, even when stationary. (This is not simply *knowledge* that the given form is projected by a three-dimensional object, since it displays unexpected and involuntary perspective reversal: what was near now looks far, with appropriate consequent size and shape "distortions.") Wallach terms this a *memory*, rather than *learning*, since only a specific "trace" is involved, and this is a long way from the creation of "trace complexes," with high generality of function—e.g., size constancy, which cannot be referred to any single memory trace.

But *why* should the rotation of a form bring its projected shadow perceptually into three dimensions?

The stationary two-dimensional pro-

jection of *any* given object is ambiguous in that there is an exceedingly large number of three-dimensional objects which will produce that projection. The *transformations* undergone by the projections as the objects rotate are unique, at least for many objects—but only if certain restrictive assumptions are made, e.g., that the projecting object must be a rigid form, etc. It is easy to say to this that, with the exception of animate creatures, most objects in our environment *are* rigid, and that this is simply a matter of distribution learning, in Brunswik's sense. However, we do not yet have any estimate of the ecological distribution of rigidity; moreover, Johansson finds with complex motions that rigidity is *not* always what is seen, even if the distal stimulus is really rigid (15).

Ambiguity (between distal and proximal stimuli) therefore must remain, but each successive momentary projection of the rotating object must, as Wallach points out, bring new information about it, and decrease the number of forms it may have and still give rise to the specific projection series. This does not immediately solve the problem of a working psychophysical correspondence, since any finite decrease of an infinite number of alternatives cannot do much toward specificity. However, if we note with Gibson that the transformation *sequence* undergone by the projection becomes *one* piece of information in the case of the usually "correct" perception of the distal stimulus, and that this requires less information to be specified than would that of describing each stage in the sequence separately—and if, as this writer has suggested elsewhere (13), that perceptual response will tend to occur which is the most economical of information—then a form of specificity returns to the stimulus-percept relationship. We must deal not with unique relationships between distal stimulus, proximal stimulus, and re-

sponse, but with relationships of varying probabilities. In Brunswik's terms, ecological validity and reliability may frequently be quite low. However, for most *normal* situations only one ecologically probable distal stimulus can yield the given proximal stimulus—if we choose adequate higher-order variables of stimulation for our analysis. Between proximal stimulus and perceptual response, then, there may be or may not be ambiguity of relationship. Where ambiguity is low, there is no problem for psychophysics; where ambiguity is high, a more probabilistic psychophysical correspondence may be in order.

A psychophysics without a punctate Constancy Hypothesis, and which employs relatively large units of analysis and tolerates probability statements of psychophysical relationship, permits the study of events of much wider scope than those we have been considering. The study of what may be termed the psychophysics of perceived causation, by Michotte (22), is only one step up the ladder from the event perceptions of motion and depth toward the study of the perception of self and others (7, 19, 25, 23). There is no intrinsic reason why such a "global psychophysics," to use MacLeod's term, cannot be applied to the study of any phenomena in which discriminational responses are obtainable. The utility of such research, of course, awaits empirical findings.

The Perception of Social Events

A start has been made in Heider's pioneer researches on the "social perception" of moving geometrical figures (cf. 12). We have seen that perceived motion might be more closely determined by change of brightness than by actual motion; here, the Constancy Hypothesis suffers some further interdimensional fracturing: objective motion

may serve as stimulus not for perceived motion, but for perceived purpose, life or animation. Under Kohler's disturbed visual conditions, animate actions became mechanical motions; contrariwise, motion configurations may give rise to the perception of action and purpose, rather than of mechanical motion (15).

Heider's "animated cartoon" had a large triangle, a small triangle, and a small circle engage in some charmingly social peregrinations through the familiar "apparent motion" of stroboscopic presentation. Naïve observers describing what they had just seen differed as to content, but agreed as to the actions in many portions of the film. Is this film only a projective technique, only a focal point around which individual antecedent experiences determine the response? If so, can at least gross similarities of experience be assumed or established? Answers can only be obtained by psychophysical research. However, we can appeal to certain at least peripherally relevant considerations.

Klein reported that when Heider's film is shown to psychotic patients (paranoid), the contents differ from those reported by normals, but the actions seen are the same. Despite the probably very great differences from individual to individual, certain stimulus variables seem important; many of us who watched Heider's film felt that, in general, uniform motion was not a compelling stimulus for perceived action, depending more upon context for its animate qualities than did nonuniform motion. Johansson reported that rows of lights moving with simple phase relations (say 180°) were described by subjects in terms of some moving mechanism; with more complex phase relations (say 90°), the motion was that of a wave, snake, etc.; with still more complex phase relations (say 45°, 135°, and 45° between the lights), almost always living motion. Tentatively, then,

perhaps nonrigid, nonuniform motion is conducive to the perception of action.

SUMMARY AND CONCLUSIONS

Empiricism-Nativism

Empiricist and nativist "explanations" were essayed quite frequently throughout the discussion, and proved equally unedifying. What one participant considered stimulus-determined (and probably innate) another considered at least partially "learned" (e.g., compare 14 and 28). One suspects that the issues of this traditional controversy have been reduced to specific questions of conditions and mechanisms which have to be settled by detailed investigation. Perhaps this is due to the fact that (at least with humans) it appears possible to find an empiricist "explanation" for almost any perceptual phenomenon; however, without concrete knowledge about perceptual learning, such general accounts are quite useless, if well-nigh invulnerable.

The Minimum Principle

The most generally acceptable summing up of Kohler's results was as follows: Two kinds of association between distal and/or proximal stimuli (including tactual-kinesthetic feedback stimulation) in our visual environment, can be at least roughly distinguished, namely invariable relations ($r \cong 1.0$) and contingent relations ($r < 1.0$). Where an invariable relationship occurs, instead of repeating it in each perceptual response, it becomes "partialled out" as norm, framework, or neutral point. As Kohler finally put it, it is as though the mathematical description of the world made by the organism takes the simplest possible form.

In Johansson's motion studies, those components in a complex moving stimulus which are common to all members of a group are "partialled out" and form a single framework in relation to which

the residual motions appear. Such unification achieves an "informational" economy since, for any given stimulus, the percept entailing the least number of changes is obtained. Objectively changing color or brightness tends to be perceived as appropriate changes in the motion of objects of unchanging color or brightness.⁵ In general, wherever a response in terms of a single unchanging distal stimulus moving in depth would be more "economical" than one in terms of continually changing relationships in two dimensions, subjects will tend to report the three-dimensional alternative. Wallach has shown that this kinetic stimulus for depth can endow even motionless projections of the distal stimulus with perceived depth, so that it seems likely that we have here one of the building-blocks by which perceptual space is achieved or modified by perceptual learning.

With respect to the perception of social events, little is yet known aside from the demonstration by Heider (and incidental observations by Michotte) that psychophysical study of such higher-order variables of stimulation seems both possible and fruitful. Heider feels that we cannot talk about those

⁵ Johansson finds that if the brightness of two motionless spots is varied with the same frequency, but 180° out of phase, the perceptual response quickly stabilizes as "a light of constant brightness moving back and forth behind the two windows." With a colored filter behind one window, a moving white light of constant intensity is perceived to move with no alteration, and the window is seen as colored. If color changes synchronize with the intensity changes, a light seems to travel back and forth behind the two windows, which are changing colors as though a bicolored curtain were being raised and lowered. That is, it seems preferable to see two such separate motion events rather than a single one which involves both motion and color changes. This appears to be quite general. Changes of stimulation which are objectively changes along other dimensions—brightness, color, etc.—tend to be ascribed to changes in the dimension of spatial motion.

motions which are perceived as actions except in terms of short, readily remembered concepts, and that the simplest action will determine the perceptual response (cf. Brunswik's "ratio-morphism"); thus, those motions most difficult to describe in simple mechanical terms appear most readily as actions.

Residual Effects of the Gestalt Revolution

After the elimination of those principles upon which agreement could not be reached, several important points remain.

1. It is frequently useful to admit subjects' statements as to what they see ("naïve" phenomenal reports) as at least contributory evidence about the percept (e.g., consider the separate courses taken by perceptuo-motor adaptation and phenomenal adaptation to visual disturbance). We cannot identify percept with perceptuo-motor manifestations, despite recent trends to do so. This is not an epistemological issue, but simply one of methodology.

2. Attention should be focused on higher-order variables of stimulation. The attempt to return to the restricted (and once presumably physiologically identifiable) "atomistic" units of pre-Gestalt days seems hopeless. Whatever measurable aspects of stimulation over space and time may be extracted and brought into correspondence with abstractable dimensions of response now constitute fair game for the investigator of perception.

3. Of all of the positive Gestalt formulations which sought to replace the Constancy Hypothesis, the principle of "simplicity" or "maximum homogeneity" has proved most general. It seems as quantifiable in terms of the objective stimulus as most of the others, and more so than some, such as the "law" of *Prägnanz*. Since existing physiological models fail to explicate the "laws" of sensory cortical organization to any use-

ful extent, the necessity for predicting response from stimulus characteristics has become progressively more urgent. The precise form of the statement and the unequivocal of the minimum principle varied with the discussants but, in one form or another, it was held by all present. What to me is its most promising approximation to date—the formulation that, other things equal, that perceptual response to a stimulus will be obtained which requires the least amount of information to specify (1, 13)—obtained agreement among Heider, Gibson, Kohler, Metzger, Johansson, and myself. Some differences existed as to whether or not this tendency is itself likely to be learned, or how precisely it should be formalized; at its simplest, it could be described as a "laziness of the perceptual imagination" (cf. Wheeler's "law of least action," etc.).

Several areas stand out as important for future investigation.

1. The problem of perceptual learning remains very poorly understood or formulated, and yet is an extremely critical area (cf. 10).

2. The perception of space, depth, and distance is frequently treated in the textbooks as a solved problem. Despite the fact that some restricted areas of precise and applicable knowledge exist, however, the basic problems in this area are completely *unsolved*, and we must launch a fresh attack on what is historically one of the oldest of the systematic problems of psychology.

3. The perception of physical and social events is an area of great promise not only for the field of perception but for potential application within and without psychology. However, it must be confessed that—aside from a very few pioneer studies in event perception, facial expression of emotions, the so-called "physiognomic perceptions," and some specific esthetic and artistic investigations (e.g. 4, 12, 25)—we know

little more about the general area than that fruitful research seems possible.

4. The study of the results of prolonged visual disturbance is critically important in the understanding of the perceptual process.

5. The *minimum principle* (the "principle," in the traditional sense, which met with most general agreement) requires more concrete and self-conscious research tests, to determine precise applicability and limits, and to avoid what Wallach and Johansson see as a potential unwarranted elevation to a metaphysical dictum.

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THE ROLE OF PRIMARY DRIVE REDUCTION IN FIXATIONS¹

ROBERT S. FELDMAN
University of Massachusetts

Maier and his students have demonstrated that subjecting rats to insoluble situations on a Lashley jumping stand leads to behavior rigidity, and that this rigidity, characterized by fixations, persists even when more adaptive responses are available to the animal (4). Moreover, the fact that animals that have been in insoluble problem situations form a bimodal distribution of learning measures during a subsequent soluble problem led Maier (5) to propose that when conflict occurs it leads to behavior that is compulsive in character, in that it is not influenced by consequences such as selective reward and punishment. Others, however, feel that Maier's evidence can or should be incorporated within a rubric of motivational and learning laws and hypotheses. For example, Mowrer (10) has suggested that the reinforcing effect of anxiety reduction is the key idea, while Miller (9) holds that the motivational force of an acquired drive of fear accounts for the compulsive persistence characteristic of behavior associated with trauma of one sort or another. These points of view were subsequently discussed by Maier and Ellen (6) who found them inadequate to account for all of their evidence relating to fixations.

This paper deals primarily with another attempt made by Wolpe (12) to account for Maier's evidence in terms of learning principles. Referring to Maier and Ellen's criticism of the anxiety-reduction explanation, Wolpe states:

There is little doubt that the experimental facts they [Maier and Ellen] give do invali-

date the anxiety-reduction hypothesis. But . . . the same facts are quite in keeping with the differently formulated hypothesis. . . . Each time a jump is forced by the air-blast, it is reinforced by reduction of the air-blast-induced drive. This is a primary drive, and its reduction is clearly overwhelmingly responsible for the reinforcement; for the platform situation is at no stage conditioned to a sufficient level of secondary drive to be able to impel jumping in the absence of the air-blast. When jumping in a particular way has thus been repeatedly reinforced, it becomes firmly established as the habitual response to the air-blast stimulus, and the more firmly it is established, the weaker does the competing alternative response tendency become (12, p. 114).

Familiarity with Maier's data suggests that this hypothesis can be questioned on two counts. First is Wolpe's statement that "the platform situation is at no stage conditioned to a sufficient level of secondary drive to be able to impel jumping in the absence of the air-blast." This statement seems to say that every response is caused by air blast, but this is not true at all. As a matter of fact, after a few days of conflict trials, very few trials require the application of air blasts or shocks to force a response in an insoluble or soluble problem situation. But this is a minor point. The more important point is that Wolpe held that the reason the rats do not give up their fixated responses for adaptive responses is that the stimulus to jumping is the air blast and not the stimulus cards. If it is true that air blast is an important variable in fixations, a simple way to find support for this possibility would be to compute both the number of times a rat was given air blast and the total duration of air blast during frustration trials, and compare these scores with some indicator of response strength.

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In an experiment by Maier and Feldman (8) three split-litter groups of rats were placed in a frustrating, insoluble-discrimination problem for different numbers of trials: one group for 80 trials, a second for 160 trials, and the third for 240 trials. All rats received 10 trials per day, and were driven by air blast if they failed to make a response within 30 seconds. This procedure generally led to persistent position responses, but if a rat developed a preference for one of the cards it was discarded, since only position-responding animals were desired for the experiment. As controls, three other groups were *trained* to form position responses by locking the window in one position 100 per cent of the time and unlocking the opposite window; one group practiced the response for 80 trials, the second for 160, and the third for 240 trials. The strength of the position response for all rats was then measured for its resistance to extinction when the problem was changed to require the learning of a card discrimination in the same apparatus. During this stage the experimenter forced the rats to respond to the correct window on trials alternated with free trials by placing his hand alongside the rat and nudging it toward the correct window. This technique insured that all rats would eventually abandon the position responses and learn the discrimination.

The measure indicating that the position response had been abandoned was the number of that trial during which the rat first makes a response different from its position response during a free trial. This number is designated as a breaking score. The results showed that the three control groups which had been *trained* to form the position responses gave up the position responses and adopted the card responses at the same rate. Thus, the duration of the practice period was not a significant variable.

The three experimental groups, however, gave up their frustration-instigated position responses after many more trials than it took for the controls; the critical ratio of the difference was in excess of six. On the other hand, the mean learning scores, i.e., scores indicating that the discrimination had been mastered, did not vary significantly for any of the groups, experimental or control.

It was seen from these results that the conflict groups were different from the trained groups in that the number of trials necessary for abandoning the position response was greater when it was acquired during frustration than when it was learned under conditions of motivation. Also, since the mean learning scores were approximately the same for all conflict and all motivated groups, it is easily deduced that the conflict groups needed significantly fewer additional trials after breaking to reach the learning criterion than did the motivated groups. This result suggested that learning took place in the discrimination problem for the conflict animals while they still expressed the position response, but that the old position stereotype² was too strong to permit the newly acquired response to come into expression.

Since the foregoing analysis assumed that the strength of a stereotype could be measured by the number of trials needed to break it, Wolpe's hypothesis would also be supported if there was a positive relationship between air-blast scores and breaking and learning scores. Therefore, product-moment correlations were computed between air-blast scores (the number of trials during the training and conflict stages during which Ss

² Attention is called to the distinction between a stereotype and a fixation. A stereotype is a consistent response to a position or to one of the windows during an insoluble problem situation. A fixation is a consistent response to a position or window when a more adaptive response is available.

TABLE 1

TOTAL NUMBER OF AIR BLASTS CORRELATED WITH LEARNING AND BREAKING SCORES, AND TOTAL DURATION OF AIR BLAST CORRELATED WITH LEARNING AND BREAKING SCORES

Group	N	Trials with Air Blast vs. Learning Scores	Trials with Air Blast vs. Breaking Scores	Duration of Air Blast vs. Learning Scores	Duration of Air Blast vs. Breaking Scores
8-day	37	-.23 (.14)*	.32 (.11)	-.07 (.15)	-.12 (.18)
16-day	37	.005 (.17)	.16 (.14)	.28 (.02)	-.07 (.17)
24-day	37	.13 (.16)	.12 (.16)	.15 (.16)	.19 (.16)

* The standard errors of the correlations are in parentheses.

received air blast) and learning scores, and between air-blast scores and breaking scores. Also, correlations were computed using total air-blast duration as the air-blast variable. Since it is desired to test the hypothesis that air blast is related to conflict-induced stereotypes, this analysis was confined to the experimental groups. The correlations with their standard errors are shown in Table 1. It is seen that practically all of the coefficients are of low order, the highest one being .32 ($\sigma_r = .11$); the reliabilities are also low. This would indicate that there can be virtually no prediction as to the persistence of a conflict-induced stereotype from the number of times an animal was subjected to air blast, or from the total duration of air blast. Since these results may reflect the possibility that rigid avoidance reactions to air blast were established after few trials and that the persistence of these reactions reached maximum levels after few more, the data of the 8-day group might be scrutinized more closely. The animals in this group received the least amount of air blast, thus minimizing the possibility that rapid and strong avoidance conditioning had occurred. Therefore, if any trend in support of Wolpe's hypothesis could be found, the animals in the 8-day group would be most likely to reveal it. But the 8-day group's relatively unreliable coefficient of .32, for the relationship between the number of trials with air blast and breaking scores, is the only

one in Table 1 that could even remotely support Wolpe's contention, whereas all the other coefficients show a trend in the direction opposite to that demanded by the hypothesis.

Furthermore, it may be recalled that in many frustration studies by Maier and others, grid shock rather than air blast was used to force responses. To explore fully the possibility that fixated responses are conditioned avoidance responses, comparisons were made between groups of rats that fixated responses and those that did not, in terms of their shock scores, viz., the number of trials accompanied by shock during the period when the responses were initially established.

In a study by Neet and Feldman (11),³ 72 rats were trained to respond on a Lashley jumping stand and then subjected to an insoluble problem for 160 trials. During a 40-trial training period and during conflict trials, Ss that failed to jump within 30 seconds were forced by applying shock to a floor grid on the jumping platform. Following the insoluble problem, all Ss were presented with a soluble brightness-discrimination problem. Testing continued until Ss made no more than one error

³ The data for this analysis were taken from a study by Neet and Feldman (11) that investigated the effect of electroconvulsive shocks on fixated responses. Their results showed that a series of 10 or 25 daily ECSs had no appreciable effect on the stability of the fixated response.

in three successive days of testing, or until they had a total of 200 test trials. The results showed that only 12 rats were able to abandon the conflict-induced stereotypes and meet the learning criterion within a limit of 200 trials. The mean number of trials that were accompanied by shock during the training and insoluble problem stages was computed for the Ss that solved the subsequent soluble problem and for those that did not within the 200-trial limit. It was found that the solvers received grid shock during a mean of 66.2 trials and the nonsolvers during a mean of 41.2 trials. This difference was significant ($p = .05$), but the difference was in the opposite direction to that demanded by Wolpe's hypothesis, that is to say, the Ss that received more shock had weaker stereotypes, judging at least by their ability to alter them.

The foregoing analysis points to the difficulty of relating the rigidity and the persistence of fixations to what appear to be obvious primary or secondary reinforcers within the insoluble problem situation described by Maier. It will be recalled that Wolpe argued quite specifically that the frequency of application of the driving stimulus (in his case, air blast) is related to the strength of the response. He stated, "Increased air-blast means increased drive with consequent greater drive reduction" (12, p. 114). Yet in our analysis of air blast and grid shock this relation does not hold at all, if one interprets "greater drive reduction" as being the primary condition for response rigidity.

Other studies point to the difficulty of specifying cues and reinforcers that relate to fixations. Feldman (1, 3) has shown that the tendency to fixate responses is not tied to discriminative cues that seem to direct them, e.g., on a Lashley jumping stand, when rats had fixated *jumping* responses to one of the cards and when on alternate trials they

were permitted to walk along runways to the windows, some of them would develop *walking* fixations to a position. Other rats fixated position responses on *jumping* trials and card responses on *walking* trials, and a group of rats that were given alternate jumping and walking trials during a solvable discrimination problem learned to walk to the correct window but persisted in a jumping fixation for a total of 200 trials. Feldman (2) has also shown that even when practice for a stereotyped response is controlled, by forcing rats to make every possible type of response in an insoluble problem situation an equal number of times, fixations still develop in subsequent discrimination problems. Maier and Ellen (7) recently demonstrated that more stereotyped responses induced by 50 per cent random reward and punishment were abandoned, and fewer fixations occurred, during a subsequent soluble problem when the correct card appeared on the fixated side 50 per cent of the time than when it appeared on that side 80 per cent of the time, even though the latter reward schedule should be more reinforcing and contribute to more learning.

Finally, consideration is directed to the common-sense argument that the rat in the insoluble problem situation has simply learned that a response to a position is as good as any other response, even though it is rewarded only 50 per cent of the time; and that if the response is made with anticipation of failure by hitting the window with the shoulder instead of the nose, punishment can be minimized. The argument further supposes that during the subsequent soluble-discrimination problem the position stereotype persists, and that the rat never tries the opposite side because that side is more likely to be more punishing since the rat has not learned to avoid bumping its nose on that side.

This explanation essentially suggests

primary reinforcing mechanisms to account for behavior rigidity, but there are two shortcomings in this approach. First, one cannot account for fixations to one of the cards on this basis, since rats jump left and right with equal facility, but always to the incorrect card. It is emphasized that in our experiments, because of barriers adjacent to the windows, rats with card fixations cannot jump so abortively as to miss the card altogether; thus they "try" and fail on every trial. It would make no sense, therefore, to say that these rats have a response that is less punishing because it is the *most* punishing, yet rats with a history of conflict trials usually do not attempt any other response.

Second, the foregoing explanation fails to account for the manifest difference between rats that acquire a conflict-induced position response and those that learn a position response by being rewarded on one side and punished on the other in a consistent fashion. In the latter case, rats learn to make the best response during position learning, but readily abandon it when changed conditions call for a response to one of the cards; the first solution has not precluded the second. If it is assumed that during the insoluble problem the position stereotype is the best adjustment possible, it is not clear why in this case this solution renders the animal incapable of solving subsequent problems. This is the crux of the matter.

SUMMARY

The data of two experiments are re-analyzed to find support for Wolpe's contention that fixations are the consequence of primary drive reduction. In one experiment, using the Lashley jumping-stand technique, three groups of rats were submitted to an insoluble problem situation for 8, 16, and 24 days, respectively. The Ss were then given a soluble discrimination problem in which they

were forced to respond to the correct window on every other trial. During all procedures, the rats were motivated to respond by applying air blast if they hesitated for more than 30 seconds during any trial. Product-moment correlations were computed for the relationship between air-blast variables (number of conflict trials accompanied by air blast, and total durations of air blast), and criteria for abandoning the conflict-induced stereotypes and learning the discrimination problem. There was no correlation of sufficient magnitude to permit a prediction of the strength of the conflict-induced response from the amount of air blast. In a second experiment, a group of 72 rats was submitted to an insoluble problem as in the first experiment. Only 12 rats were able to solve a subsequent soluble problem. During all procedures, rats were motivated to respond by grid shock if they hesitated for more than 30 seconds on the jumping platform. Comparing the frequency of shock between the Ss that abandoned the conflict-induced stereotype and solved the problem and those that failed, it was found that the rats that *solved* had significantly more shock during the conflict trials. These results were considered as failing to support the notion that fixations are the consequence of the reduction of drive induced by air blast (or shock).

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ANALYSIS OF EXPLORATORY, MANIPULATORY, AND CURIOSITY BEHAVIORS

WILLIAM N. DEMBER¹ AND ROBERT W. EARL

University of Michigan

A class of behaviors apparently not motivated by the commonly accepted homeostatic drives, nor by learned accretions to these drives, has come under considerable experimental investigation in recent years. Such behavior has been variously labeled exploration, manipulation, or curiosity. Various labels have been applied to the conditions for arousal of this behavior, chief among them novelty and complexity. While this behavior has been discussed at length by Nissen (16) and Harlow (8), there is available as yet no comprehensive theory to account for it. Hebb's "short-circuiting" (9) has been offered as an explanatory concept at the neurophysiological level. It is our purpose in this paper to suggest a psychological structure into which such behavior and the conditions of its arousal might fit.

CHARACTERISTICS OF THE BEHAVIOR

In any attempt at classification of events there are two dangers: (a) the inclusion in the same category of events which only superficially belong together, and (b) categorization as different events those which are only superficially dissimilar. In this paper we are more likely to be guilty of the first error.

We would assert that those behaviors labeled "exploration," "curiosity," etc., belong to the general class of behavior, *attention*. Functionally, this behavior brings the organism into contact with certain portions of its environment rather than others. Such behavior differs from attention of the textbook variety only in the degree of locomotor involvement. One may attend to a loud noise without very gross motor ad-

justments, whereas a rat, to make contact with the end box of a T maze, ordinarily must get there on its own four feet.

This point of view enables us to consider in a single system such diverse phenomena as binocular rivalry, manipulatory behavior of the monkey, exploratory activity of a rat in a maze, and certain aspects of the aesthetic and intellectual behavior of human beings. An obvious advantage of this scheme is that it affords a multitude of situations for inducing and testing hypotheses, and a truly comparative approach.

In what follows we shall label as *attention* the behavior under consideration, and mean by it any behavior, motor or perceptual, which has as its end-state contact between the organism and selected portions of its environment. Our major problem is to specify the conditions of organism-environment interaction that determine *what* gets selected. Those stimuli which are the objects of attention we label *goal stimuli*. We would distinguish *goal stimuli* from *means-end stimuli*: the latter represent the routes by which goal stimuli are attained. It is with respect to the selection of means-end stimuli that game theory and decision theory are concerned. Our primary concern is with the selection of goal stimuli.

THE AROUSAL OF ATTENTION

In this section we shall discuss two experimental techniques that have been found successful in arousing attention. We shall argue that common to these distinct techniques is the production of *change*, whose relation to attention was early recognized by Pillsbury (17).

¹ Now at Yale University.

Temporal Change in Stimulation

There are many experiments which utilize a temporal change in stimulation to arouse attention. Illustrative of these is an experiment by Dember (6). Rats on Trial 1 were allowed to see into the two arms of a T maze but were prevented by means of glass partitions from entering either arm. During the 10-minute exposure period one arm was black, the other white. The animals were removed from the maze, the partitions removed, and one arm was changed so that both arms were either white or black. The rats on Trial 2 were reintroduced into the maze and, faced with a choice between two arms equal in brightness, 17 out of 20 entered the arm whose brightness had been changed from Trial 1 to Trial 2. By the sign test, the above results are significant at the .001 level of confidence (one-tailed). Undergraduate students have repeated Dember's experiment several times since then with almost identical results.

Results analogous to these have been reported by Thompson and Solomon (18) and Berlyne (1, 2) with rats, and recently by Welker (20) with chimpanzees. It is to conditions like the above, involving operations that produce a temporal change in stimulation, that the term "novelty" most aptly refers.

It should be noted that a temporal change in stimulation arouses attention only if the change produces a discrepancy between what is observed by the subject and what is expected. The motivational implications of discrepancy have, of course, been emphasized by Hebb (9) and McClelland (12).

The experiments referred to above utilize change in stimulation over discrete periods of time. In one of Breese's many studies on binocular rivalry (4) a continuous change was effected in one of the two stimuli by means of a device which moved the stimulus over the field. The moving stimulus was found to dominate attention almost completely.

To summarize, an important experimental technique for arousing attention is to present the organism with stimulation which is discrepant from the organism's expectation. This may conveniently be accomplished either by exposing the organism to a stimulus and then changing the stimulus in some manner, or by producing a continuous change in the stimulus.

It is obvious that the indicator response, from which attention is inferred, may vary considerably, depending on the species studied and on the experimental conditions. We especially would emphasize that in the present analysis no distinction is made genotypically between "approach" and "avoidance" behavior. Hebb's chimpanzees, who find the isolated head of one of their species so discrepant as to be emotionally disturbing, nevertheless are certainly attending to it.

Spatial Change in Stimulation

An analysis similar to that above may be applied to the case of goal stimuli whose ability to arouse attention is produced by spatial dishomogeneity. Breese (4), for example, found that one of the two fields in a binocular rivalry experiment could be made dominant simply by adding lines to it. Similar results are reviewed by Vernon (19). The experiments of Montgomery (15) are also pertinent here. His rats learned to choose the arm of a Y maze leading to a Dashiell maze within which they could wander, as opposed to the arm leading to a plain goal box. Welker's chimpanzees preferred the more spatially heterogeneous of a pair of stimuli (20).

Spatial heterogeneity is a characteristic to which the term "complexity" has been often applied. Complexity, like novelty, may be transformed into a psychological variable by defining it in terms of change in stimulation, in this case spatially induced.

While the distinction between the

temporal and spatial factors by which change is induced is useful in describing some of the experimental techniques available for producing goal stimuli, psychologically it is an unnecessary distinction. Spatial change implies some sort of scanning process on the part of the individual; the scanning of a spatially heterogeneous stimulus is equivalent to movement by the stimulus, over time, past the "stationary" individual. In this sense spatial heterogeneity has the same effect psychologically as temporal change of stimulation.

SYMBOLIZATION OF CHANGE

A useful set of symbols for abstracting the above ideas is offered in Coombs's measurement theory (5). The symbol Q_{hj} is the measure of a stimulus, j , on some attribute for an individual, i , at the moment, h . The symbol C_{hj} , analogously, is the measure of an individual, i , on some attribute for a stimulus, j , at the moment, h . The measures and attributes in this theory are not properties of the physical stimulus events or objects, but are defined by means of the discriminatory responses to such objects or events on the part of observers who are different from the experimenter. While the C values in Coombs's theory are ordinarily applied to such attributes as "arithmetic ability," "attitude toward the church," etc., we may generalize the concept of C value to include expectancy. Thus, an individual at time h may have a "brightness measure," not in the sense of intelligence or amount of light reflected from his face, but in the sense that he comes to a stimulus with an expectancy (C_{hj}) about the brightness value (Q_{hj}) of that stimulus. In the Dember (6) experiment, for example, the rat approaches the choice point on Trial 2 with a brightness measure for each arm, C_{hj} and C_{hk} . These measures correspond to the traces left by the two arms on the exposure trial.

A third measure derives from the interaction between C and Q : $P_{hj} = |C_{hj} - Q_{hj}|$. P_{hj} symbolizes the absolute value of the discrepancy between the expected and the observer; P_{hj} measures the novelty of a stimulus. In the example above, if j is the arm whose brightness was changed between trials, and k is the arm that remained the same, then it may reasonably be assumed that for the rat $P_{hj} > P_{hk}$.

Implied in this analysis is the assumption that, despite the brightness change, the maze arm, j , has retained its identity. It is the identity of j which makes meaningful the process implied in $|C_{hj} - Q_{hj}|$. This assumption, as Berlyne (1) points out, is made by McDougall (13) in his discussion of the curiosity instinct. The problem of stimulus identity has been emphasized recently by Kivy, Earl, and Walker (10), who demonstrated the importance of context in the production of satiation: exposure to a brightness equal to that of one of the arms of a T maze did not influence subsequent choice, if that exposure occurred other than in the T-maze choice point itself.

It is possible to apply to complexity the symbolization suggested above for novelty. Consider as an example of a complex stimulus a pattern of vertical black and white stripes. Suppose the subject scans the pattern from left to right, and the acceptance angle of his scan is one stripe width. First he sees a black area which has a certain Q value and which leaves the individual with a corresponding C value. He scans farther to the right and sees white. Now his C value, aroused by the preceding scan, will interact with the Q value of the present scan to yield a P value—if stimulus identity is preserved from first to second scan. It is as though the subject says to himself on the first scan, "This stimulus is black," and then as he scans farther "No, it's white."

The measure of complexity, P , is in

effect a measure of response variability; the interpretation of this measure is conceptually equivalent to the interpretation of "amount of information." For example, to the extent that discrepancies can arise as a stimulus is observed, there will be uncertainty, or information in the stimulus. In the spatial case, the amount of information will decrease as one scan yields a C value which equals the Q value of the succeeding scan, rather than the Q value of the immediate scan. A stimulus is redundant to the extent that it fails to offer the possibility of discrepancy.

Note that the loss of information, in the above sense, results from the modification of the relevant C values of the individual. Operationally, the stimulus is just as heterogeneous as before, but the individual through contact with it has changed. Unfortunately, information theory cannot solve the problem of the objective measurement of complexity, although it may contribute to an eventual solution.

It should now be clear that both temporal and spatial change are operations which are potentially novelty- or complexity-producing, but are not necessarily so. The amount of information arising from any experimental technique might be assessed a priori by methods such as Fitts *et al.* (7) have suggested. But the measure of complexity, P , arising from the application of that technique can only be recovered from the discriminatory responses of the subject under study. Under some conditions it is possible to make a good guess about the relative amounts of complexity in a set of stimuli, provided one has sufficient knowledge concerning the traits and past experiences of the subjects. Thus, it is quite likely that the striped stimulus above will be more complex for most subjects, at least upon first contact, than a spatially homogeneous stimulus. Similarly, the changed

arm, j , in the Dember (6) experiment should be more complex (or novel) for the animals than the unchanged arm, k . But it must be emphasized that these are merely hunches on the part of the experimenter. To make possible experimentation amenable to unequivocal a priori prediction, techniques are required that permit the assessment of stimulus complexity for the subject to be used. It is to this problem that some of Coombs's (5) recently developed measurement methods are perhaps applicable—at least where the subjects possess some degree of linguistic ability.

A MOLAR ANALYSIS

We may consider the Q s, C s, and P s of the preceding discussion as elements in a more molar system whose units are sets of these elements. Thus, we might assign to a stimulus, on each of its attributes, an over-all complexity value arising from some interaction among the elementary within-stimulus discrepancies: symbolically, $Q'_{hj} = f(P_1, P_2, \dots, P_n)$. In this sense we speak of a stimulus as having a complexity value, Q'_{hj} , on each of its attributes. Note that Q'_{hj} is a dimensionless quantity, as is pointed out by Miller (14) in his discussion of "amount of information."

The individual may also be assigned a complexity value, C'_{hj} , on each attribute, corresponding in a generalized sense to his "ability" on that attribute. Just as an individual at a given time has a certain ability with respect to arithmetic problems—some he can "pass," some he cannot—so, too, he has an ability with respect to paintings, music, literature. C'_{hj} is the complexity in a stimulus on some attribute which the individual at time h prefers.

Analogous to the molecular C values, C' changes with experience. Unlike the C values, however, the C' change is unidirectional: with experience, C' takes on increasing values. The individual

can only get *more able*.² Now the only stimuli which can change C' are those with Q' values greater than C' . The individual in effect is "paced" by stimuli. It is intuitively reasonable, however, that there be only a limited range of Q' values which are effective in changing C' . Some stimuli may be too complex to act as pacers. A person just learning French will not improve on being exposed to Proust or Baudelaire, though he may be able to evaluate their work as much more complex than that in his elementary text.

We would define as a *pacer* a stimulus with a Q' value in the acceptable range above C' . Now, a set of stimuli will be attention-arousing if that set contains a pacer. We postulate that under that condition, *the individual will apportion his attention among the stimuli in the set in proportion to their similarity to the pacer, with the modal amount of attention applied to the pacer.*

As the individual has experience with the set, his C' value will increase, until such time that the set no longer contains a pacer. At that time the set will no longer have the ability to change C' , and thus will have lost its attention-arousing property.

Given free choice, the individual will cease responding to the set. Forced to respond to it, he will experience boredom, and under certain circumstances a great deal of misery, as in the perceptual isolation experiments of Bexton, Heron, and Scott (3). Given no assistance from his receptors, he may resort to creating new stimuli—i.e., he may hallucinate, dramatically as in the isolation experiments, or less so, as in autokinesis, binocular rivalry, and reversible figures.

CONCLUDING COMMENTS

The ideas presented in this paper are neither novel nor complex, but we be-

lieve that some such scheme as we offer is necessary if we are to progress past the stage of merely demonstrating behavior apparently embarrassing to orthodox Hullians. Of course, by insisting that complexity is a psychological variable, assessable meaningfully only by psychological measurement techniques, we emphasize the difficulty of research in this area. But such a position is the only realistic one.

We further complicate the issue, and get closer to reality, by making attention a function not only of stimulus complexity but also of the individual's complexity—which itself changes with experience. We know nothing about the parameters of change, or how they vary across attributes. There is, however, the suggestion in an experiment by Krech and Calvin (11) that some aspect of change is related to verbal intelligence, which at least indicates that research on this problem is possible.

A most important source of research difficulty which we have allowed ourselves to face lies in the distinction previously mentioned between means-end stimuli and goal stimuli. Any research on goal stimuli must include techniques of insuring that it is goal stimuli that are being studied, or techniques of partialing out the means-end aspect of the stimuli. In working with human subjects, one must also find ways of eliminating ego involvement—of creating situations where the subject is truly free to behave in accordance with his evaluation of the stimuli as goal objects per se. A complete theory, of course, would specify the conditions for accomplishing this, but until that is available the experimenter's ingenuity must suffice.

One final point should be made concerning the concept of complexity. We have deliberately avoided defining complexity as an attribute, but rather have made it a dimensionless measure of a stimulus on any attribute. This enables us to take what seems the reasonable

² Regression of C' results from anxiety; the individual in this paper is always nonanxious.

position that a stimulus may have a different measure of complexity on each of its attributes; analogously an individual may have a complexity value on each attribute: on independent attributes these values may change independently. Thus, one's complexity for music may increase by the day, while one's complexity for literature remains, in the absence of the appropriate experience, at the level of Mickey Spillane.

SUMMARY

We have classified exploration, manipulation, and curiosity in the category of attention. Two experimental techniques for arousing this behavior have been analyzed. We have argued that these two techniques have a common psychological basis, change or discrepancy, which may be symbolically represented in Coombsian terminology. The possible application of Coombs's measurement techniques to the experimental measurement of the amount of psychological discrepancy has been suggested. Finally, at a molar level, we have defined the conditions under which a set of stimuli will have attention-arousing value and under which this property is lost. Underlying the molar analysis is the idea that the dynamics of attention are based on the ability of stimuli to increase the psychological complexity of the individual who perceives them.

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CONDITIONS AFFECTING THE AMOUNT OF INFORMATION IN ABSOLUTE JUDGMENTS

EARL A. ALLUISI¹

Laboratory of Aviation Psychology, The Ohio State University

Certain concepts of information theory (9, 15) have been used in measuring man's ability to make absolute judgments. It has been observed, for example, that as the amount of input information to the human is increased, the amount of information transmitted by the human (both in bits/stimulus) will increase at first and then level off at some asymptotic value (10). This asymptotic value is the maximum amount of information transmitted ($\text{Max } H_i$) under the specific set of experimental conditions employed. When conditions are optimized for a specific task, $\text{Max } H_i$ will attain its highest possible value; it may then be called the *channel capacity for x* , where x is the specific psychological function or task studied (e.g., the visual discrimination of brightness, the auditory discrimination of loudness, etc.). When x is an absolute discrimination task, the anti-logarithm of the channel capacity may be treated as an estimate of the span of absolute judgments of that stimulus dimension, i.e., as an estimate of the number of categories of stimulation that can be discriminated absolutely with an arbitrarily small percentage of errors (6).

Spans of absolute judgments may differ for different stimuli, however, so that the channel capacity obtained with one

stimulus or task may be higher or lower than the value obtained with another stimulus or function. Man's *Channel Capacity* may then be defined as the highest of all the channel capacities for specific tasks; capitalization might even be used, as here, to differentiate this one "highest possible value independent of task" from the several "highest possible values for specific tasks."

These two concepts should not be confused. A recent review by Miller (10) was concerned with Channel Capacity (capitalized), whereas the present paper is concerned with channel capacity (uncapitalized). Specifically, the present paper is intended as a review of the available data indicating under what conditions of experimentation an estimate of channel capacity (rather than of $\text{Max } H_i$) might be obtained.

In scope, this review will be limited to a consideration of data concerning the absolute judgments of stimuli lying along simple dimensions. First, auditory stimuli will be considered, then visual stimuli, and finally a brief section on individual differences will complete the paper.

AUDITORY STIMULI

Pollack (12) had O s identify frequencies by assigning numbers to different tones. The tones differed in equal logarithmic steps in the range from 100 to 8000 cps. Knowledge of results was given O after each response; i.e., after each response O was told which of the tones had been presented. The amount of information transmitted by the av-

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erage O^2 was found to increase linearly up to about 2 bits/stimulus, and then to approach an asymptote of about 2.5 bits/stimulus, as the number of alternative tones within the stated range was increased from 2 to 14 (i.e., as input information was increased from 1 to 3.8 bits/stimulus). This $\text{Max } H_i$ of 2.5 bits/stimulus seems to imply that the span of absolute judgments of frequency differences in auditory stimuli is in the neighborhood of about five categories.

Pollack also found, however, that information transmission varied as a function of both the total range of frequencies used and the spacings used between stimuli. For eight tones, the mean amount of information transmitted with three closely spaced series was only 1.73 bits/stimulus, whereas it was 1.90 bits/stimulus with three widely spaced series. When the entire range from 100 to 8000 cps was used, the mean amount of information transmitted with eight tones was

2.00 bits/stimulus, but when only part of this range was used it dropped to 1.725 bits/stimulus. The actual values for the eight-tone data ranged from 1.6 to 2.1 bits/stimulus for the different conditions (12, p. 747, Fig. 5).

The finding that information transmission varies as a function of the spacing used between the stimuli has been corroborated by Garner (4). In a study that required Os to identify different intensities of a 1000-cps tone by assigning numbers to the different tones, he concluded that "... greater information transmission will occur when the stimuli are spaced according to a criterion of equal discriminability" (4, p. 238).

The finding that information transmission varies as a function of the range of stimulation used has been definitively corroborated by Schipper (14), whose experiment had two conditions. In one condition, the number of intensities of a 1000-cps tone judged by Os was varied from 2 through 4, 6, 8, and 10 tones with a fixed 5-db interval between stimuli. In the second condition, stimuli were selected at equal db intervals to divide a fixed 45-db range into 4, 6, 8, and 10 tones; this fixed range was equal to that used with 10 tones in the first condition. The amount of information transmitted by the average O in the first condition was found to vary from 0.47 to 1.29 bits/stimulus as the number of tones judged varied from 2 to 10. In the second condition, no significant differences in the amounts of information transmitted by the average O (about 1.26 bits/stimulus) were found for the different numbers of tones.

These results imply two things. First, there is an optimal spacing of stimuli that will maximize the measured $\text{Max } H_i$ for a given stimulus dimension. There are theoretical grounds for postulating that when the stimuli are spaced according to a criterion of equal perceptual discriminability, the value of Max

² There are two ways of computing an "average" of the information transmitted per O . Assume that there are m stimuli, n responses, and k Os. Then, (a) the amount of information transmitted by each of k Os could be computed separately from each of the k , $m \times n$ stimulus-response matrices. The arithmetic mean of these k values is referred to in this paper as the "amount of information transmitted by the average O ." On the other hand, (b) the data of the k Os could be pooled into a single $m \times n$ stimulus-response matrix from which a single "average" information value could be computed for all k Os. This value for pooled data is referred to in this note as the "average amount of information per O ." These two specific phrases are used in the present paper only where the mode of computation is definitely known.

It might be noted that the latter value can never exceed the former for the same set of data. It may, however, be less than the former to the extent of the constant differences among Os. In fact, the difference between these two "average" values might be used as a quantitative estimate of individual differences, i.e., an estimate of the reduction in information transmission that occurs as a function of the constant differences among Os.

H_t obtained will be higher than with any other spacing (6), i.e., adjacent stimuli so spaced should represent equal confusion tendencies, so that the over-all tendency will be to minimize total errors and thereby maximize the information transmitted by reducing the total equivocation.

The second implication is that the asymptotic value of information-handling performance obtained with any given stimulus dimension is dependent upon the range of stimulation used in obtaining that Max H_t . The greater the stimulus range, the closer will Max H_t approach the channel capacity for the function being studied. This means, further, that even when optimal stimulus spacing is used, the inherent judgmental accuracy of O is independent of the number of stimulus categories used experimentally only insofar as a fixed range of stimulus variation is employed.

VISUAL STIMULI

The range of stimulus variation used experimentally has also been found to affect information transmission with visual stimuli. Eriksen and Hake (2) had O s judge the sizes of visually presented squares. A statistically significant increase in the amount of information transmitted by the average O was found to occur when the range of sizes was increased from 40 mm. sq. to 80 mm. sq.; the change amounted to about .2 bit/stimulus and represented an average change of about 10 per cent in the Max H_t estimated. The number of stimulus categories used in the study was also varied over 5, 11, and 21 different sizes within each range, and O used 5, 11, and 21 response categories with each of the different numbers of stimulus categories. A significant interaction was found between the number of stimulus and response categories. In general, the amount of information transmitted by the average O was lower for a given

range when the number of response categories was fewer than the number of stimulus categories.

In another study, O s interpolated visually between two scale markers defining a .16-in.-long horizontal interval. The number of pointer positions used within the interval as stimuli for O to judge varied over the values 5, 10, 20, and 50. Approximately equal amounts of information were transmitted with all numbers of pointer positions 10 or greater. From these data of Hake and Garner (7), a Max H_t of about 3.25 bits/stimulus could be estimated. Because the Max H_t appears to be a function of range, however, this estimate of nine or ten categories should not be taken as an invariant measure of the span of absolute judgments of interpolated visual position. Rather, it should probably be taken as a specific estimate applicable only to the length, or subtended visual angle, of the specific horizontal interval employed.

The effect of stimulus spacing has recently been investigated in a study of hue judgments. Conover (1) had O s judge the hues of colored papers, selected from the Munsell 50-hue maximum saturation series, by assigning numbers to the different hues. Two different experimental conditions were used, but in each condition the same range of stimulation (a complete color circle) was employed. In one condition, ten O s made absolute judgments of the 25 different stimulus categories obtained by taking every other hue in the Munsell series. Then, the 25 Munsell hues were scaled for equal discriminability, and 16 equally discriminable hues were selected from this scale. A different group of 21 O s made absolute judgments of these 16 hues in the second condition of the experiment.

The amount of information transmitted by the average O was 3.486 bits/stimulus in the first condition, and

3.524 bits/stimulus in the second. The difference between these two values was not statistically significant. This, however, does not invalidate previous findings that spacing affects information transmission, for the scaling of the 25 stimulus hues had indicated that the stimulus categories were roughly of equal discriminability as originally selected from the Munsell series. That is to say, the equal discriminability scale formed with the 25 stimulus hues was approximately linear (see 1, p. 39, Fig. 5), and so the stimuli in both conditions of the experiment were spaced essentially with equal-discriminability intervals.

The estimated span of absolute judgments of hue in the Conover study (1) was about 11 categories, or 3.5 bits/stimulus. This is a little higher than the eight or nine categories (3.1 bits/stimulus) estimated from the data of Eriksen and Hake (3), but it is in close agreement with the estimated number of absolutely identifiable spectral hues (between 10 and 12 hues) reported by Halsey and Chapanis (8). The estimate would not apply, of course, to just a portion of the range of hues, but only to the full circle of hues.

Information transmission has been found to be affected by other variables in addition to range and spacing. Sidorsky and Slivinske (11, pp. 30-37) had *O*s judge the inclination of a line of .1-in. length. The line could appear at any of the 72, 5° positions in the 360° of a circle. They found that when knowledge of results was given, the amount of information transmitted by the average *O* was 4.87 bits/stimulus (about 29 categories), but that when no knowledge of results was given it dropped to 4.66 bits/stimulus (about 25 categories). With four *O*s, the difference between these two means was statistically significant at the $p < .01$ level of confidence. The presence or absence

of an outline circle around the inclination symbol did not affect information transmission (11, pp. 38-39), nor did variations in the line length of the inclination symbol from .1 through .3 in. (11, pp. 40-41).

Ellipse-axis ratios between a straight line (ratio of minor-to-major axis equal to zero) and a full circle (ratio of minor-to-major axis equal to unity) were judged, both with and without knowledge of results in another study (11, pp. 50-56). Twenty stimulus categories were used in equal axis-ratio steps of .053 between zero and unity; the major axis was $\frac{7}{16}$ in. long in all cases. With the major axis horizontally oriented, the amount of information transmitted by the average *O* was 3.24 bits/stimulus with knowledge of results, and 3.13 bits/stimulus without knowledge of results. With the major axis oriented vertically and no knowledge of results, the amount of information transmitted by the average *O* was 3.18 bits/stimulus. The differences among these three means were not statistically significant at the $p = .05$ level of confidence with five *O*s. Approximately nine categories are to be associated with each mean as the estimated span of absolute judgments of ellipse-axis ratios.⁸

⁸ These studies in the absolute judgments of hue, inclination, and ellipse-axis ratio are unique in that they each cover a full possible range of stimulus variation. In a sense, then, the spans of absolute judgments estimated from these studies are not range-bound as are the values estimated from other studies. The spans for hue, inclination, and ellipticity appear to be in the neighborhood of 11, 29, and 9 categories, respectively.

For inclination, the value of 4.87 bits/stimulus exceeds the largest channel capacity reported by Miller (10, p. 86) for a unidimensional stimulus (3.9 bits/stimulus for positions in an interval). It might be argued, however, that inclination is not psychologically a unidimensional stimulus; two studies (11, pp. 30-37, 42-49) have indicated that judgmental accuracy is about the same in each of the four quadrants of the circle.

One other study is pertinent to this discussion of the effects of knowledge of results, spacing, and range conditions on the amount of information transmitted in making absolute judgments. Sidorsky (11, pp. 57-69) had *O*s make absolute judgments of the sizes of small circles of light within the range from $\frac{3}{64}$ to $\frac{17}{64}$ -in. diameters. When 15 categories were used with stimulus intervals of $\frac{1}{64}$ -in. diameter, the amount of information transmitted by the average *O* was 2.66 bits/stimulus (about six categories) with knowledge of results, and 2.37 bits/stimulus (about five categories) without knowledge of results. For five *O*s, the difference between these two means was statistically significant at the $p < .02$ level of confidence. Moreover, when eight categories were used over the same range with stimulus intervals of $\frac{3}{64}$ -in. diameter, the amount of information transmitted by the average *O* without knowledge of results was 2.51 bits/stimulus (about six categories). When eight categories were used over half the range (from $\frac{7}{64}$ to $\frac{14}{64}$ -in. diameters) with stimulus intervals of $\frac{1}{64}$ -in. diameter and no knowledge of results, the amount of information transmitted by the average *O* was 1.74 bits/stimulus (about three categories). The differences between this last mean and the other three means were all statistically significant at the $p < .01$ level of confidence. Equal discriminability scales constructed for the four conditions of the study indicated that relative discriminability remained about the same over comparable ranges of the scale.

The results of this study of size judgments indicate that information transmission is influenced more by the range of stimulus variation than (a) by the number of stimulus categories, at least above some minimum, or (b) by the spacing between stimulus categories within wide limits. This further emphasizes that in order to obtain an es-

timate of channel capacity for any given stimulus dimension, the widest possible stimulus range must be used; otherwise, the obtained asymptotic value of information-handling capacity should be regarded as a $\text{Max } H$, specific to the stimulus range used experimentally.

INDIVIDUAL DIFFERENCES

The effect of individual differences on information transmission may be illustrated in two ways. First, the information-transmission value computed from the pooled data of k *O*s⁴ will be equal to, or less than, the arithmetic mean of the k information-transmission values computed from the unpooled data of k *O*s.⁵ The difference between the two estimates of "average" information transmission is a measure of the gain in information transmission that occurs if the specific *O* transmitting the message is known. Illustrations of the different estimates obtained with these two methods of computation are shown in Pollack (12, p. 746, Fig. 3) and in Garner (5, p. 374, Fig. 1).

The second way in which individual differences in information transmission are illustrated is in the variation of the k values computed from the unpooled data for k *O*s. For example, the amount of information transmitted by ten *O*s making absolute judgments of 25 Munsell hues was found to vary from 3.185 to 4.215 bits/stimulus (9 to 19 categories); for 21 *O*s making absolute judgments of 16 hues with equal discriminability spacings, it varied from 3.117 to 4.000 bits/stimulus (9 to 16 categories) (1, p. 33, Table VI). Similarly, for 12 *O*s making absolute judgments of the intensity of a 1000-cps tone with ten stimuli within a 45-db range (5-db intervals

⁴ The "average amount of information transmitted per *O*," as used in this paper.

⁵ The "amount of information transmitted by the average *O*," as used in this paper.

between stimuli), the amount of information transmitted varied from 0.73 to 1.64 bits/stimulus (1.7 to 3.1 categories) (14, Appendix I).

The data of these studies indicate that measures of information transmission, like most psychological measures, vary from individual to individual. Therefore, estimates of the span of absolute judgments should only be based on data collected from adequate samples of individuals if those estimates are to be taken as representative of the population.

SUMMARY

This paper reviews the available data pertaining to the conditions of experimentation that appear to affect the span of absolute judgments. In addition to individual differences, the ability to make absolute discriminations among a set of stimuli appears also to be a function of the following four experimental conditions:

Range of stimulus variation. The amount of information transmitted with (or the estimated span of absolute judgments of) any given stimulus dimension is dependent upon the range of stimulus variation used experimentally. In general, the greater the stimulus range, the greater the amount of information transmitted in bits/stimulus. Range seems to be the largest over-all determinant of information transmission with a given stimulus dimension.

Spacing of stimuli. The maximum amount of information transmitted with a given stimulus range and a given number of stimulus and response categories may be reduced by use of nonoptimal spacings between stimulus categories.

Number of response categories. The number of response categories seems to interact with the number of stimulus categories used experimentally. In gen-

eral, when the number of response categories is fewer than the number of stimulus categories, the amount of information transmitted appears to be lower than when the number of response categories is equal to (or greater than) the number of stimulus categories.

Knowledge of results. The amount of information transmitted when knowledge of results is given to *O* after each response appears to be greater than when no knowledge of results is given.

In conclusion, man's channel capacity for any given task is defined as the highest possible asymptotic level of performance (highest $\text{Max } H_i$) obtainable with that task. To measure channel capacity experimentally, optimal conditions of range, spacing, number of response categories, and knowledge of results would have to be used. Otherwise, the value obtained will be something less than the channel capacity—a $\text{Max } H_i$ specific to the experimental conditions employed.

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DISCRIMINATION LEARNING THEORY: UNIPROCESS VS. DUOPROCESS¹

HARRY F. HARLOW AND LESLIE H. HICKS²

University of Wisconsin

A clear-cut differentiation may be made between uniprocess and duoprocess learning theories. Uniprocess theories assume that a single, basic physiological process, be it inhibition, excitation, or X , underlies habit formation and that a single interrelationship exists between this single process and reward, on the one hand, and nonreward on the other. The duoprocess learning theories assume that underlying learning there are two basic physiological processes, commonly called "excitation" and "inhibition," excitation being the resultant of reward, and inhibition the resultant of nonreward.

In this paper, interest is limited to the processes underlying discrimination learning. Many have advanced theories of discrimination learning, but, to date, Spence is the only writer to formulate a theory that generates quantitative predictions of the course of learning of a discrimination problem involving discriminanda with qualitative differences, the commonest type of laboratory discrimination learning problem. His theory clearly specifies two processes to account for the learning. Before dismissing other quantitative theories it might be pointed out that Hull (3), also a two-process theorist, limited his treatment of discrimination theory to situations in which the discriminanda vary quantitatively along a single stimulus dimension. Gulliksen and Wolfle (1) have proposed a theory broad enough to cover discriminanda having qualitative

differences, but application of the theory to qualitatively varying stimulus objects would require preliminary experimentation to scale the objects psychologically, an impractical procedure in the present situation.

Any duoprocess theory of discrimination learning must postulate that reward and nonreward have differential effects on learning. To account for the demonstrated phenomena of discrimination learning, it is probably also essential to duoprocess theory to assume, as did Spence (7) in his 1936 theory, that the differential effects are not constant throughout the course of learning but vary in some systematic manner. Spence postulated that reward leads to an increment in excitatory tendencies toward the stimulus components varying in amount per reinforcement during the learning of the problem so that the sequence of increments might form a parabolic curve. Thus, increments in excitatory tendency would be small early in learning, increase to a maximum in the middle course, and then decrease late in learning. Similarly, he postulated that nonreward results in a decrement in excitatory tendencies, regarded as "an active negative process, inhibition, which, adding itself in algebraic fashion to the positive excitatory tendencies, results in lowered strength values" (p. 430). He believes that successive nonreinforcements produce decrements or inhibitory tendencies increasing in amount as the strength of the response increases. Spence assumes that the successive decrements form a linear curve, although the actual form might be curvilinear. He was careful

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² Now at Howard University.

to point out that the exact forms of the curves of excitatory and inhibitory tendencies are not important to the theory, but it is implicit to the theory that the forms of these functions differ.

Unfortunately, in the two decades that have passed no one has apparently attempted a direct test of this crucial theoretical problem. It appears obvious that a number of tests favorable or unfavorable to the outlined duoprocess learning theory can be made, but there is some question whether or not the theory *per se* can be tested.

A direct measure of the relative or absolute increment or decrement produced on specified trials in an object discrimination problem is difficult or impossible because the patterns of the preceding rewards and nonrewards become so complex. Thus, no one has been able to assess the strengthening and weakening effects of reward and nonreward for Trials 7, 17, 27, and 37 or for any other selection or succession of trials. Furthermore, individual learning constants enter into the equations for predicting increments and decrements. Another difficulty in the theory outlined by Spence, as far as testing it is concerned, lies in the fact that the absolute or relative strengthening or weakening effect of reward or nonreward on any particular trial is influenced by original object and position preferences which the animal brings to the discrimination situation.

Spence's theory was designed, of course, to account for the learning of individual problems, but, because of the difficulties cited, a test of the theory in the single-problem situation is probably impossible. In a multiple-problem, or learning-set, situation, with a group of subjects, some of the difficulties can be controlled: object and position preferences can be balanced, and individual differences in the learning equation lose power through averaging. Likewise,

patterning of reward and nonreward trials can be controlled, so that the effects of reward and nonreward can be assessed at any stage of learning-set development. But the introduction of the multiple-problem technique introduces new variables whose possible influences might be a source of concern to those aligned with the Spence theory.

Sufficient data on learning-set development exist to justify, in view of the writers, the use of learning-set technique to test the operation of one vs. two processes in discrimination learning, and, moreover, they would hold that the results have relevance to the Spence theory of discrimination learning even though these results cannot be regarded as a pure test of the theory.

Object discrimination learning-set formation is conventionally described by plotting the percentage of correct Trial-2 responses of a series of problems. The resultant curve is typically either exponential or S shaped, depending upon the nature of the subjects, the pretest conditions, and the difficulty of the kind of object discrimination problem chosen. The performance increments resulting from the successive trials of a series of problems mirror, or can be made to mirror, increments resulting from successive trials of a single discrimination problem. If two processes operate in the learning of learning sets, differential performance curves should appear on Trial 2 of Problems 7, 17, 27, and 37 if the first trial of these problems was rewarded in the one instance and not rewarded in the other. If the successive Trial-1 responses of a set of problems are made correct or incorrect an equal number of times within succeeding small groups of problems, and if the learning of learning sets results from the two processes of "excitation" and "inhibition," the learning set curves for Trial 2 of this set of problems should be of one form for the problems that had

Trial 1 rewarded and of another form for the problems that had Trial 1 unrewarded. If uniprocess learning theory is correct, the two learning curves should have the same form throughout their course.

There is presumptive evidence that Trial-2 performance on successive discrimination problems is actually homologous with the successive-trial performance on a single discrimination problem. Meyer (4) has demonstrated that the scores made on Trial 2 of successive problems fall on the intraproblem learning curve, and that equal amounts of problem practice translate segments of the learning curve equal distances along a trial-unit abscissa. Although the Meyer measures were made on discrimination reversal problems, the results must hold for discrimination learning, since the only difference between these two problems for the monkey relates to the relatively unimportant stimulus-perseveration error-factor (2). Certainly the intimate interrelatedness of intraproblem and interproblem learning suggests a communality of basic mechanisms.

The actual problem which the present experiment is designed to test, however, is the operation of uniprocess vs. duoprocess learning in the acquisition of object discrimination learning sets.

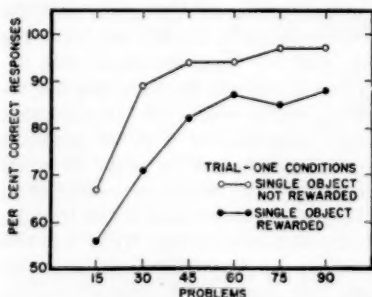


FIG. 1. Discrimination learning set curves based on Trial 2 responses.

METHOD

Eight adolescent rhesus monkeys (No. 190, 191, 193, 195, 196, 197, 198, 199) served as subjects. They had been previously tamed, adapted to the test room and apparatus, and used in a latency experiment involving displacement of a single wooden block (5). The animals were tested in the Wisconsin General Test Apparatus with a two-hole tray. Stimuli were 360 pairs of objects differing multidimensionally.

The standard noncorrection trial procedure was utilized (2), with a raisin as the reward on correct trials. All problems were six trials in length, and 12 such problems were given each animal each day for 30 days. The position of correct and incorrect stimulus objects was controlled by use of balanced positional sequences (2).

The daily set of 12 problems for each *S* consisted of 3 problems of each of four kinds, differing only in first-trial conditions, as follows:

Condition A. A single object was presented over one of the two holes on the tray on Trial 1 and was not rewarded. On Trials 2 to 6 this object continued as the incorrect object paired with a second object which was consistently rewarded.

Condition B. A single object was presented over one of the two holes on the tray on Trial 1 and was rewarded. On Trials 2 to 6 this object continued as the rewarded object paired with a new object, consistently not rewarded.

Condition C. Two objects were presented on Trial 1, and response to either was not rewarded. The object which the monkey selected on Trial 1 was the incorrect object on Trials 2 to 6.

Condition D. Two objects were presented on Trial 1, and response to either was rewarded. The object which the monkey selected on Trial 1 was the correct object on Trials 2 to 6.

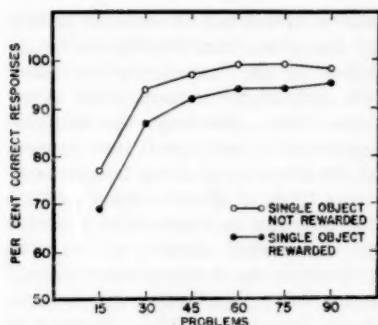


FIG. 2. Learning set curves based on Trial 2-6 responses.

Since Conditions A and B were the only conditions to balance stimulus and position preferences on Trial 1, they were regarded as the conditions critical for the test of uniprocess vs. duoprocess theory, and primary attention is directed to them.

RESULTS AND DISCUSSION

The performance of the monkeys on the 90 successive Trial-2 responses obtained for each animal under the A and B conditions is presented in Fig. 1. There is no indication that the two curves differ in form or that they fail to parallel each other continuously. Furthermore, there is no indication that either curve changes its rate of gain relative to the other curve at any point.

The fact that performance is consistently better under Trial 1 nonreward than Trial 1 reward is immaterial, since it is differential curve form rather than a constant rate difference which would be expected if reward and nonreward showed differential strengthening and weakening effects during the course of learning.

The obtained constant rate differences had been expected, empirically, in accordance with the findings of Moss and Harlow (6), and also theoretically, since nonreward minimizes response

shift and reward maximizes it (2). Such data as exist (2) would indicate that the response-shift error-factor for Condition-B problems would either not be materially reduced during the course of the experiment or would be reduced only near the end. Thus, it would either not affect the form of the curve, or it would affect it only near the end of the experiment, in this case reflected as a gradual rise of the Trial-2 Condition-B curve as compared with the Trial-2 Condition-A curve.

The performances of the monkeys on the 90 successive mean Trial 2 to 6 responses made under Conditions A and B are presented in Fig. 2. Again, the two curves appear to be highly similar, if not identical, in form. It is possible that at the last plotted point the Condition-B reward curve is accelerating relative to the Condition-A nonreward curve, but the differences are well within the range of experimental error, and would not be unexpected in terms of the described operation of the response-shift error-factor (2).

The learning curves for all four training conditions during Trials 2 to 6 are depicted in Fig. 3, even though it was considered that Conditions C and D are not critical tests because of lack of control over stimulus preferences. If neither object is rewarded on Trial 1, the mon-

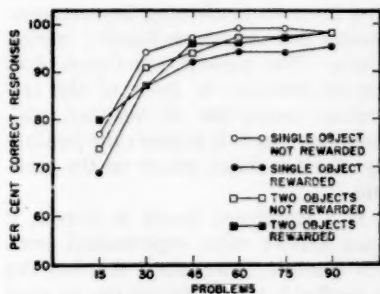


FIG. 3. Learning curves for the four conditions of first-trial reward.

key's preferred object, as indicated by Trial-1 choice, becomes the negative object, and the animal must choose the nonpreferred object on subsequent trials. If both objects are rewarded on Trial 1, the preferred object is the correct one for the trials that follow. The high performance under Condition D at the initial plotted point, and at no other point, was predicted from the fact that stimulus preference is an important factor influencing choice early in learning, and then decreases sharply in importance after the early stages of object discrimination learning-set formation by rhesus monkeys (2). It may be noted that the composite curves for the A and B, single-object, conditions, and for the C and D, double-object, conditions are almost identical at all points after Problems 1 to 15.

It is quite obvious that the present experiment is not a direct test of Spence's discrimination learning theory, because the present experiment measures learning in the learning-set situation. The data appear, however, unequivocally to support a uniprocess interpretation of learning, because reward and nonreward operated in a parallel and constant manner throughout the entire course of the experiment in influencing learned performance.

No single experiment can possibly settle a theoretical problem as crucial as that involved in choosing between duoprocess and uniprocess learning mechanisms. The present experiment does present evidence in favor of the uniprocess conception of learning, and, more important, it suggests the possibility of other direct attack on the problem.

If a uniprocess theory is correct, it must survive other experimental tests. For example, if we accept that learning is ascribable to a single process, we must assume that the generalization curves to the rewarded and to the unrewarded

training stimuli will be identical, or that any departure from identity can be explained by the operation of independently defined mechanisms, such as response shift. Similarly, the temporal decay which should result from increasing the intervals of delay between successive trials of discrimination learning problems must be constant for rewarded and unrewarded stimuli, or must be attributable to independently defined mechanisms. Specifically, if we measure the learning on Trial 1 in terms of performance on Trial 2, and introduce four intertrial delay intervals such as 10, 20, 30, and 40 seconds, the delay functions to initially rewarded or unrewarded stimuli should be identical in form. Similar results should also obtain in delayed-response testing.

On the basis of preliminary studies we know that adequate testing of uniprocess theory in these described situations is difficult because of the rigid controls which must be established over past experience, stimulus preference, and the operation of the powerful response-shift error-factor. Our preliminary studies consistently show greater learning to the unrewarded stimulus in all situations; the results are indeterminate in regard to the form of the generalization curves or the temporal decay functions. The results are, however, of such a nature as to suggest that definitive tests can be made by improved precision of testing and better understanding of the factors which operate to produce error in the nonspatial discrimination learning situation.

If a uniprocess learning theory is accepted, it is interesting to speculate concerning the fundamental nature of this mechanism. There are three possibilities: the mechanism may be excitatory, inhibitory, or undefined—a mechanism *X*. Wisdom or cowardice would dictate mechanism *X*. Hebb apparently prefers excitation, and the authors prefer

inhibition. At the present time there appears to be no direct test among the three mechanisms, and, this being true, choice is dictated merely in terms of which thesis best integrates existing learning data. Analysis of the nature of errors during learning and a review of the phylogenetic literature on learning have influenced the authors in their choice. The problems and perils of inhibitory learning theory are beyond the scope of this paper, but the assumption of a position offers new approaches in visualizing learning and the development of new families of problems.

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CREATIVE ABILITIES IN THE ARTS¹

J. P. GUILFORD

University of Southern California

In 1950, the writer set forth some hypotheses concerning the component abilities that were believed to be needed to account for creativity (1). These hypotheses were developed by way of preparation for systematic studies of this phenomenon by a combination of experimental and factor-analytic approaches. In order to complete the setting of these studies, which is important in preparation for some of the things to follow, something must be said about the scope of the studies of creativity.

The aptitudes project² has not been confined to the study of creativity, but has investigated all types of thinking abilities, including also those traditionally known as reasoning abilities, and those we chose to include under the headings of planning and evaluation. This inclusiveness was fortunate, for we find that the whole area of thinking abilities or functions is rich with interrelations and parallels. The understanding of some parts of this total area is very helpful in understanding others.

On the other hand, the studies of creativity proper, up to the present, have been more limited than they might have been. In setting up hypotheses concerning the component abilities in creativity, we were guided mostly by the kinds of creative activity recognized as such in scientists, engineers, inventors, and in supervisory and administrative personnel; in other words, types of per-

sonnel that are of concern in the military setting. We did tolerate the general hypothesis that the abilities that make these kinds of personnel creative might be the same as those that make the painter, the composer, the writer, and others creative, but we did not reject the contrary hypothesis, for we had no basis for doing so. To be sure, it would be a simpler outcome to find that the same qualities of fluency, flexibility, and originality, for example, account for performances of artists and scientists alike. But in our research we have never been very strongly influenced by the goal of simplicity. We have seen that all too often the compulsion of this goal has been unfortunately restricting of the investigator's outlook.

We did favor the notion that creativity, whatever its range of application, is by no means a unity but is rather a collection of different component abilities or other traits. Our results have definitely supported this general point of view. They also suggest the hypothesis that in the areas of the performances of the graphic artist and the composer, at least, we shall find new factors; factors that are distinct from those that are important in creativity of scientists, technologists, and managers, yet that are parallel to them. It is from the information concerning abilities that we have already investigated that we can deduce something about creative abilities that we should find to be important in the arts. The artistic, creative abilities that I shall mention are thus mainly hypothetical, but I should say that there are better precedents for these hypotheses than for those presented in 1950. We have made a beginning toward re-

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lating some of the known factors to the art of writing. It is hoped that the presentation of the hypotheses in this article will stimulate other investigators to test them in connection with other arts as well.

In general, the support for the expected factors thought to be important in the arts lies in the systematic nature of the whole collection of thinking factors, also the memory factors. Enough of the thinking and memory factors are known for us to see the lines of a system. A conception of the entire collection of intellectual factors has been presented in a recent issue of the *Psychological Bulletin* (2). It will pay us to review briefly the features of that system that are relevant in support of factors that are predicted to be important in the arts.

THE SYSTEM OF INTELLECTUAL FACTORS

One of the significant principles of the system is that the factors fall into three parallel groups, depending upon the kind of material involved in the activity. Let us think of them as being in three parallel columns. Psychologists have had a long-standing recognition that different abilities are involved in verbal tests on the one hand and nonverbal tests on the other. In nonverbal tests the psychological material dealt with by the examinee is in the form of figures, letters, numbers, or other symbols. Our project results show that we must make a further distinction within the nonverbal area. The consequence is that the intellectual factors tend to come in groups of three parallel abilities or traits. For example, there are three abilities for seeing relationships between things. One of them applies to relations between perceived figures. A second has to do with relations between meanings or concepts. There is a third relations-seeing factor that has to do with the ability to see relationships between such materials as letters, numbers, or

other simple symbols. In the latter case, it is neither their figural nor their meaning properties that determine the relationships; it is some other property. We have called this category of factors the "structural" group. In everyday life, the structural type of thinking is perhaps most evident in mathematics. It does not appear that the structural factors have much significance for the arts, as such, and that we shall have to look for the significant artistic abilities among the figural and conceptual factors.

One or two additional comments will help to show just where the artistic-creative factors fit into the general scheme of intellect. The thinking factors seem to fall into three general groups of another kind, in a cross classification. This grouping is based upon the kind of *action* performed. There is a group of *cognition* factors, a group of *production* factors, and a group of *evaluation* factors. We become aware of the things with which we are confronted; we produce something of our own in response to that awareness, or something that it calls for; and we evaluate our products of thought. A total creative act involves all three aspects—cognition, production, and evaluation. A schematic view of all the classes of intellectual factors is shown in Figure 1.

In view of the active nature of creative performances, the production aspects or steps are most conspicuous and

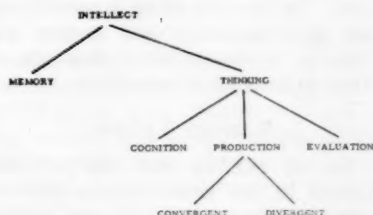


FIG. 1. Diagram of the major categories of intellectual factors and their logical relationships.

probably most crucial. Among the productive-thinking abilities another logical distinction appears. With some productive-thinking factors, and the tests that measure them, thinking must at some time converge toward one right answer; the significant type of thinking involved has been called "convergent" thinking. With other productive-thinking factors and their tests, thinking need not come out with a unique answer; in fact, going off in different directions contributes to a better score in such tests. This type of thinking and these factors come under the heading of "divergent" thinking. It is in divergent thinking that we find the most obvious indications of creativity.

This does not mean that convergent thinking and divergent thinking never occur together. They frequently do, in a total act of problem solving. Creative steps are necessary in solving new problems. Actually, we can hardly say there is a problem unless the situation presents the necessity for new production of some kind. Factors are abstractions of components from total activities. Some of the components are recognized as being more creative than others, for example, qualities of fluency, flexibility, and originality. These come under the category of the divergent-thinking aspect. While they may contribute toward reaching one right answer where that is demanded, they are more obvious in activities where this is not the case. In the arts there is usually *no* one right answer. Some answers are regarded as merely better than others. There is a matter of evaluation.

FLUENCY FACTORS

Let us consider first the potential factors in the more obvious creative areas of fluency, flexibility, and originality. Our project results thus far have clearly indicated four fluency factors, two flexibility factors, and one original-

TABLE 1
A TABLE OF THE PRODUCTIVE-THINKING
FACTORS OF THE DIVERGENT-
THINKING TYPE

Type of Result Produced	Type of Content		
	Figural	Structural	Conceptual
Words		<i>Word fluency</i>	<i>Associational fluency</i>
Ideas			<i>Ideational fluency</i>
Expressions			<i>Expressional fluency</i>
Shifts	<i>Flexibility of closure</i>	<i>Adaptive flexibility</i>	<i>Spontaneous flexibility</i>
Novel responses			<i>Originality</i>
Details	<i>Elaboration*</i>		<i>Elaboration*</i>

* At present regarded as the same factor, but future results may indicate two separate factors.

ity factor. For the most part, our tests of these factors fall into the conceptual column. Table 1 is given to show the whole matrix of productive-thinking factors and their interrelationships.

Let us consider the fluency factors first. Two of them, *word fluency* and *associational fluency*, have to do with the production of single words. Tests of *word fluency* are best characterized by the fact that the words produced must meet specified structural requirements, such as listing words beginning with a certain letter or words ending in a certain suffix. Meanings or concepts are of no importance. The *word-fluency* factor thus falls in the structural column. Parallel to it, the *associational-fluency* factor is measured by tests that involve listing words having some meaningful requirement, such as listing synonyms or opposites for a stimulus word. To complete this triad of factors, there should be one involving the production of letter combinations that satisfy certain *figural* requirements, such as the activity of producing monograms or other artistic effects with words.

The factor of *ideational fluency* stands alone at present in another incomplete

triad. It is the ability to produce rapidly a succession of ideas meeting certain meaningful requirements. The number of words produced in each response may be one or several. For example, tests of this factor may call for the listing of things round, of ideas about a man going up a ladder, of titles for a story plot, or of predictions of consequences of events. Quantity is important but quality is not.

A parallel factor in the figural column would be an ability to produce a variety of artistic ideas in limited time. Rough ideas for themes, rough sketches, and the like would be sufficient output so far as this ability is concerned. At this point we must face a question that has general significance, beyond the *ideational-fluency* areas. This is the question whether ability to produce numerous ideas in the graphic arts is the same as the ability to do so in music.

There is a precedent at one place in the system of intellectual factors for a distinction between visual and auditory functions. This occurs among the memory factors. We have an apparent triad of memory factors in all of which the learning and retention of associative connections between contents is the important thing, and a second triad in which memory for the contents themselves is essential. In the latter triad, there is an ability to remember the substance of meaningful verbal material. Parallel to it are two factors, rather than one, having to do with remembering substance in figural form. There is a factor of *visual memory*, and this is separate from a factor of *auditory memory*. The latter involves memory for such things as melodies and rhythms. We may regard melodies and rhythms as auditory figures.

The separation of two figural-memory factors, visual and auditory, suggests that a similar distinction may be found elsewhere in the system, and hence pos-

sibly in the area of the production of figural ideas. This would mean that the ability to produce ideas in the graphic arts is distinct from the ability to produce ideas in music. With this separation of factors, we should have four ideational-fluency factors, not forgetting the one in the structural column, rather than the one factor already known. There could even be a fifth ideational-fluency factor connected with the kinesthetic sense, also in the figural column. This ability would presumably be of importance to the successful choreographer or creative acrobat.

The fourth known fluency factor is called *expressional fluency*. Thus far confined to verbal tests, this factor is recognized as an ability to put ideas into words. Tests requiring the putting of words together in appropriate, connected discourse are best measures of the factor. The distinction of this factor from *ideational fluency* is support for the common observation that it is one thing to have an idea and it is something else to be able to put it into words.

Three of the known fluency factors should go a long way to account for talent for writing. *Ideational fluency* should give the writer something to write about; *expressional fluency* should enable him to put it into appropriate words; and *associational fluency* should help to find words with the right shadings of meanings without the help of word-finding aids.

Is it likely that there are factors to complete a triad of expressional factors? The concept of expression is surely not foreign to the arts. Having a graphic or a musical idea is short of the total creative production. Putting the idea into appropriate organizations of figural material would be necessary to complete the process. And possibly, again, we shall find that expression in graphic form depends upon a different ability

than expression in musical form, just as both differ from expression in verbal form.

A general conception of creativity that calls for so many distinctions and separations of function may be somewhat surprising to readers. Why, after all, should there not be much in common between having ideas in the graphic arts, in music, and in writing? Why should there not be much in common in expressing ideas in the different media?

Notice that I have referred to such abilities as being distinct; not necessarily as being independent or uncorrelated. I suspect that there *is* something in common among parallel factors. This should not preclude their statistical and experimental separation, provided that performances of these different kinds or with different materials are not perfectly correlated when correction is made for unreliability. I suggest that we proceed to find out whether the factors are statistically separable and, if so, whether they separate along the line hypothesized or along some other lines. Then we can more appropriately raise the question about their interrelationships.

There is some evidence from everyday observation to lend support to the separateness of the expressive abilities. For example, the production of a popular song often involves the collaboration of a composer and a lyricist. To be sure, some individuals do both successfully. Actually, the correlation could be zero between these two performances, and yet there would be by chance *some* individuals who show high status in both respects. The production of a motion picture, in which musical, graphic, and conceptual ideas are commonly expressed in blended combination, is a synthetic task of specialists, just as, more and more, even single cinema characters rep-

resent synthetic blends of talents of different performers.

FLEXIBILITY FACTORS

The two flexibility factors that we have found differ in more than one respect. One difference is that one factor is found in verbal tests and the other mostly in nonverbal tests. One therefore belongs in the conceptual column and the other in the structural column, or possibly it cuts across structural and figural columns; we are not sure. The other difference is in the role that each factor plays or the degree of compulsion vs. freedom involved.

The conceptual-flexibility factor is called *spontaneous flexibility* because the examinee shows flexibility on his own initiative; the test items do not necessarily require it. It is possible that this quality is a temperamental trait or a motivational trait rather than an ability; a disposition to avoid repeating oneself, or an urge to vary one's behavior. If this is true, the trait might be accounted for under the Hullian concept of "reactive inhibition," or under the concept of a general psychological refractory phase, or under the concept of satiation. Being quite general in its determination of behavior, such a trait might serve as the basis for very fanciful, creative imagination wherever it is found, for example, in artist and scientist alike.

The other factor in this area we have called *adaptive flexibility*, because it is important in the solution of problems—particularly those that require the discarding of familiar or habitual methods and striking out in new and unusual directions. We have more recently expected to find three factors of this kind, but thus far have not found them, probably because our test variations have been inadequate to effect their separation. To the extent that there are prob-

lems involved in the arts, this kind of ability or trait would seem to play a significant role. It remains to be seen whether an adaptive-flexibility factor that is unique to figural material is required.

ORIGINALITY

The one factor of *originality* seems to be rather general, in one sense at least. That is, it is indicated by varied tests; tests that require unusual or uncommon responses, remote associations or connections, or clever responses. The use of an unusual variety of tests has provided much opportunity for a separation into two or more originality factors along the lines of such differences. All the tests have been verbal, or have involved verbal meanings in some way. We have as yet provided no opportunity for finding a triad of originality factors, distinguished along the lines of the materials involved.

There is a possibility that the factor of *originality* will prove to be fundamentally a temperamental or motivational variable. For example, it might be a general set to be unconventional or to avoid repeating what other individuals do. A single trait of this kind could be expected to cut across material categories. There would then be one originality factor; not a triad. Some of our future research will be directed along these lines with regard to originality as well as with regard to flexibility.

We have already made a beginning toward relating fluency, flexibility, and originality factors to temperamental and motivational variables. At present, it does not appear that any of them can be accounted for on the basis of such nonaptitude variables as we are exploring in this connection. This leaves the way clear for testing the hypothesis that there are complete triads of factors in these three areas.

OTHER FACTORS RELATED TO CREATIVITY

In 1950, in addition to factors of fluency, flexibility, and originality, it was hypothesized that there would be an ability to see problems, an ability to analyze, an ability to synthesize, and an ability to redefine or reorganize objects of thought. The hypotheses concerning analyzing ability and synthesizing ability were rather decisively discredited by the results. In spite of the opportunities for such unitary abilities to make themselves known, they failed to appear. This does not mean that, in thinking, no such activities as analyzing and synthesizing take place, for too many activities can be described as such. The unitary abilities that individuals have in common and that have a bearing upon success, however, are better described otherwise. This kind of conclusion is not unique. No one would deny that we indulge in activities that are properly called thinking, and yet there is no generalized unitary ability to think. There are many thinking abilities, as previous discussion has demonstrated. An all-too-common error in psychology has been to assume that because a range of phenomena can be subsumed under a single name there is therefore a unitary function. Every such assumption must be tested by empirical procedures.

A factor was found that could be defined as the ability to see problems. It is a cognition factor rather than a production factor. It proved to be much less general than was originally expected, being confined to seeing defects and deficiencies in such practical matters as everyday gadgets and implements and in social institutions and practices. The tests that measure the factor have been exclusively verbal. Will the use of comparable nonverbal tests give us completion of a triad of problem-seeing factors? This hypothesis should be tested.

To the extent that the artist has problems, we may suppose that there are individual differences in ability to recognize them. The problem might be in the form of the need for a theme or a particular kind of theme, or in the form of expression or treatment, or in the use of techniques and implements. Among these would be problems involving figural properties of things. The triad hypothesis would lead us to expect little correlation between the ability to detect *such* problems and the ability to detect the kind represented in verbal tests.

The factor of redefinition involves the ability to desert one interpretation or conception of use of an object, or part of an object, and to adapt it to some new function or use. For example, the cover glass of a watch can be removed and used as a condensing lens to start a fire. How readily can the individual arrive at such a transformation? How good is he at improvising in similar situations in general? This variable is a divergent-thinking factor that involves the production of a shift of meaning of an object. Are there parallel factors involved anywhere in the arts?

Actually, there is a factor of *visualization*, which seems to be to the figural column what *redefinition* is to the conceptual column. The factor of *visualization* is the ability to think of changes or transformations of a figural kind in visually perceived objects, or in objects visually thought of. The relation of such an ability to work in the visual arts can be readily imagined. There might even be such a factor in the auditory field, enabling a composer or arranger to produce variations on a theme with changes in use of phrases so radical that they take on new values or functions.

A factor of evaluative ability was hypothesized, not as a contributor to the production of creative results but as a means of determining whether such re-

sults are good, suitable, correct, or adequate. In our investigation of this area of thinking, we gave ample opportunity for more than one evaluative-ability factor to emerge. There are different bases or criteria by which a product is judged. One is its logical consistency with known facts. Another is its less-than-logical consistency with other experiences. There are also different kinds of products to be judged, depending upon the kind of materials involved. We included tests with both figural and verbal material. At the time of the study of evaluative abilities, the third category of structural materials had not yet been recognized.

We found three general evaluation factors. *Logical evaluation* is an ability to judge products on the basis of their logical consistency with given facts. A factor called *experiential evaluation* seemed to fit the picture of an ability to judge products in terms of consistency with past experiences. In the interpretation of this factor, if the emphasis is placed upon ability to make use of past experiences in the act of judging, it could be a rather general ability. If, however, emphasis is placed upon the past experiences, we face the real possibility of many common factors of this kind, depending upon the more or less coherent bodies of information that people acquire, for example, mechanical, mathematical, and so on. As for the rest, the use of experience would be a rather specific matter.

A third factor, which was called *perceptual evaluation*, is of uncertain generality. It can readily be hypothesized that there are as many perceptual-evaluation factors as there are coherent areas of perceptual functioning. The variety of psychophysical judgments is, of course, almost unlimited. The tests that defined our *perceptual-evaluation* factor emphasized comparisons of lengths of lines and total sizes of figures. The factor we found may therefore have been

the more limited *length-estimation* factor that was previously known.

The whole area of evaluative abilities is still largely unexplored. I have hinted that we may expect to find a very large number of rather narrow evaluative-ability factors. As for evaluation in the arts, presumably the *logical-evaluation* factor would not apply. Experimental-evaluation abilities might account for aesthetic tastes in terms of aesthetic values. Perceptual-evaluation abilities would have much bearing on the acceptability of art forms, visual, auditory, or kinesthetic. They would perhaps be numerous and also generally of narrow scope.

The factors mentioned thus far are those we originally regarded as belonging in the creative category. Recognizing that some aspects of planning are also creative, certain newly obtained factors in that area could also be regarded as creative. But as the system of the intellectual factors developed, cutting across our original categories of reasoning, creativity, and planning, these category concepts have shrunk in importance. Furthermore, it became more apparent that, in the creative activity of everyday life, other abilities than those regarded as primarily creative also play roles to some degree. For example, is it not likely that a large vocabulary is desirable for the creative writer? Should not the developer of ideas in descriptive geometry be able to think readily in terms of visual-spatial arrangements? These two examples imply the usefulness of the factors known as *verbal comprehension* and *spatial orientation*, respectively. Norman C. Meier has also emphasized the finding that individuals with recognized artistic talents are unusually able to observe and to remember clearly things they perceive (3). This implies a high degree of the factor known as *visual memory*, an ability to remember visual content. The factor of

auditory memory may play a similar role for the composer.

Thus, a great number of primary mental abilities that would not be regarded as creative abilities nevertheless play their roles at times in creative work. We might say that minimal levels of such abilities are desirable, if not necessary, for success in various artistic activities. We might say that to that extent these are necessary but not sufficient conditions for creative production. The factors of fluency, flexibility, and originality, and the like, are not only necessary but, when possessed in adequate amounts, are sufficient. All of this, of course, assumes adequate motivating conditions, also. In the process of surveying the resources of creative artists of any kind, therefore, whether this is for the sake of better understanding of talent or for the practical purposes of prediction and guidance, it would be well to ask whether any of the intellectual factors may play a significant role, and where and how, if so.

SUMMARY

1. It is hypothesized that creative artistic talent is not a unitary or uniform commodity but is to be accounted for in terms of a large number of factors or primary mental abilities. From what is already known, we should expect that the creative abilities of artists will be found to involve some factors other than those among creative abilities in fields such as science and management.

2. Of the known factors, certain ones, of fluency, flexibility, and originality, are the most obviously creative abilities. All of them come under a general class of factors known as productive-thinking abilities and in a subclass of divergent-thinking abilities.

3. A developing system of all the intellectual factors indicates the relationships of the more creative factors to one another and to other factors. From

certain relationships and parallels, unknown factors that are probably important in the arts can be hypothesized with some confidence.

4. A full account of complete creative-artistic performance involves evaluative abilities and abilities that are not primarily creative, many of which are already known.

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A GRAPHICAL DESCRIPTION OF ROTE LEARNING

BENTON J. UNDERWOOD

*Northwestern University*¹

It has often been pointed out that a learning curve for a group of *Ss* does not adequately reflect the fluctuations in the performance of the individual *S*. A curve representing the combined performance of a group of *Ss* usually shows a smooth increment in performance as a function of trials or time, whereas a curve for a single *S* shows increments, decrements, and plateaus. However, since these fluctuations occur at different points in time for different *Ss*, they apparently average out to show a smooth progression for the group. A group curve could reflect these fluctuations if (a) a given fluctuation is lawfully related to earlier performance of an *S*, and if (b) common reference points for all *Ss* could be used to measure this lawfulness. Hayes (1) has shown how this descriptive problem might be handled with data derived from the discrimination learning of rats. The present paper shows one way in which a group curve will reflect the individual fluctuations for data from rote learning. The method, with exemplary data, will be given first and then certain limitations and implications of the description will be pointed out.

Consider the situation when *S* is given a serial or paired-associate list and is asked to learn this list to a criterion of one perfect trial. Different *Ss* will require different numbers of trials to attain this criterion of mastery. Two general ways have been used to show the group learning curve for such data. One method is the Vincent technique, in which the total learning period for each

S is divided into equal trial parts (usually tenths), and the total number of correct responses in each part counted and then averaged for all *Ss*. Another method is the trials-to-criteria curve. In this method the number of trials required by each *S* to attain each successive criterion of 1, 2, 3 . . . *N* correct responses on a single trial is determined, and these values are averaged for all *Ss* for each criterion. The present method of describing the rote-learning process starts with a trials-to-criteria curve.

The trials-to-criteria curve is a series of points representing the earliest point in learning at which the average *S* achieved each criterion. If a representation is desired which combines maximum performance and earliest achievement of that maximum, no better method could be devised. But if it is assumed that learning involves a series of systematic fluctuations or oscillations, it can be seen that the trials-to-criteria method is "picking off" performance at the top of the oscillations. Melton (2) noted this a number of years ago, showing that, following attainment of each successive criterion, many *Ss* "fell back" to lower levels of performance. Now it would seem that if the series of points defining the trials-to-criteria curve represent the peak of fluctuations, a series of points showing the lowest or poorest performance after each peak might represent the nadir or bottom of the oscillations.

As illustrative data we have used the records for 100 *Ss* learning a difficult, ten-item serial list. This list was composed of nonsense syllables of low association value and of high intralist similarity. An average of 42.6 trials was required to learn the list to one perfect

¹ This work was done under Contract N7onr-45008, Project NR 154-057, between Northwestern University and the Office of Naval Research.

trial (3). The following data were obtained from the learning records.

1. Average number of trials required to reach each successive criterion.

2. Average lowest performance shown after attaining each successive criterion.

3. Average number correct on the trial just *before* the trial on which *S* attained each criterion. These values could be obtained for all criteria except one and two correct responses. Many *Ss* got two correct responses on the first anticipation trial, so that there is no "before" trial. Two *Ss* got three correct on the first anticipation trial, and one got four. Therefore, the *N* for the "before" trial for three correct responses is 98, for four correct 99, and for all others, 100.

4. Average number correct on the trial just *after* the trial on which *S* attained each successive criterion. All *Ss* are represented for these values up through six correct responses. After seven, *N* is 99, after eight, 98, and after nine, 86. These reduced *Ns* result from

Ss' "jumping" late criteria. For example, *S* may get eight correct on one trial and ten correct on the next, so that no record is available for the trial after attaining nine correct. Since these *Ss* who jump are usually fast-learning *Ss*, the mean number correct on the trial after achieving nine correct responses is probably slightly low. If *S* jumped from, say, two correct to four correct on successive trials, and if on the trial after getting four correct he again got four correct, he was recorded as having achieved four correct on the trial after reaching the criteria of two, three, and four correct.

These four sets of data are plotted on a single graph in Figure 1. The abscissa represents trials, and the ordinate the mean number correct. The upper dotted curve, therefore, simply represents a trials-to-criteria curve, and shows the usual smoothness. The open circles represent performance on the trial immediately after each criterion, and the closed circles performance on the trial just be-

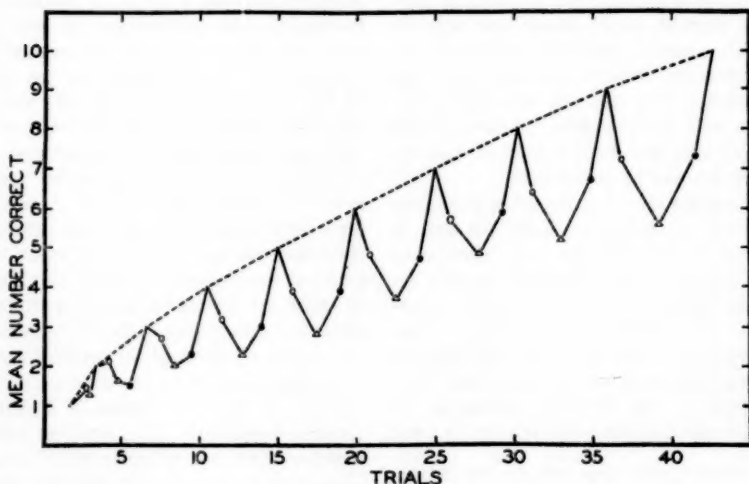


FIG. 1. Trials-to-criteria learning curve and a cyclical curve from the same data. See text for complete explanation.

fore each criterion is reached. For each criterion these are placed one trial after and one trial before, respectively, according to the abscissa scale. Finally, the points shown by the triangles represent the lowest performance shown after attaining each criterion; these triangles have been placed halfway between the before and after points.

It may be noted that the mean lowest performance after the criterion of two correct responses was achieved (the lowest point between two and three correct responses) is not as low as the "before" three correct performance. This apparent contradiction results again from S's jumping early criteria. Suppose S got 2, 5, and 6 items correct on three consecutive trials. His performance on the trial just before getting three correct is recorded as two. But, his lowest point after achieving two correct (the lowest point between two and three correct responses) is five correct. The "before" measure must always be lower than the criterion it precedes; the low point need not be.

The following characteristics of the solid-line curve may be noted:

1. Except after the criterion of one correct, performance falls at some point after attaining each criterion. As would be expected from plotting such curves for less difficult lists, the easier the list the longer are such falls delayed, i.e., they occur only after late criteria. If the average number of trials to learn is less than the number of items in the list, it is possible that no falls after any of the criteria would be observed.

2. The higher the criterion, the further the fall and the longer or more prolonged is this lower performance. If the amount of fall is changed to percentage of the immediately preceding criterion, however, the amount of fall so expressed is relatively constant for all criteria after the first two.

3. The performance on the trial just before attaining a criterion is consistently lower than on the trial just after reaching that criterion. However, the performances on the trial just after attaining a criterion and on the trial just before attaining the next are roughly equal.

It should be repeated that the list on which Figure 1 is based is a difficult one; easier lists will show less extreme fluctuations. Furthermore, with easier lists it will only be possible to get the low points between successive criteria and maintain relatively full *N*s. These low points are critical for describing the bottom of the oscillations; the performance on trials just before and just after attaining each criterion can be omitted without changing the basic picture.

It seems evident that Figure 1 gives quite a different picture of the learning process from that of the typical learning curve (the dotted curve). However, it remains to be seen whether or not the cyclical curve has any useful or special theoretical significance. Those who use *oscillation* as a theoretical construct may find some comfort in these data; it is conceivable that certain physiological cycles might be shown to be related to the performance curve. An examination of the differences between fast and slow learners for the present data show that slow Ss fall further after reaching each criterion than do fast Ss. These slow Ss fall further on the trial immediately after each criterion and their low point is lower. However, their performance on the trial just before attaining each criterion is as high as the fast-learning Ss.

SUMMARY

It has long been known that group learning curves do not reflect the oscillations present in the learning records

of the individual S. A way of coordinating these oscillations for a group of Ss is presented for rote-learning data. The resulting graph shows systematic cycles in the performance curve.

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ON PERCEPTUAL READINESS¹

JEROME S. BRUNER

Harvard University

About ten years ago I was party to the publication of an innocent enough paper entitled "Value and Need as Organizing Factors in Perception." It was concerned with what at that time was the rather obscure problem of how extra-stimulus factors influenced perception, a subject then of interest to only a small band of us—Gardner Murphy, Nevitt Sanford, Muzafer Sherif, and a few others. Obviously, Professor Boring is quite right about the mischievousness of the *Zeitgeist*, for the appearance of this paper seemed to coincide with all sorts of spirit-like rumblings within the world of psychology that were soon to erupt in a most unspirit-like torrent of research on this very topic—perhaps three hundred research reports and theoretical explications in the ten years since then. F. H. Allport (1) and M. D. Vernon (81) have each recently had a fresh look at the field, sorting out the findings and evaluating the theoretical positions, and they have done superb service. Their labors free me to pursue a more relaxed course. What I should like to do in this paper is to set forth what seem to me to be the outlines of an approach to perception congruent with this body of new (and often contradictory) findings and to sketch out what appear to me to be the persistent problems still outstanding.

¹ The present paper was prepared with the invaluable assistance of Mr. Michael Wallach. I also benefitted from the comments of Professors W. C. H. Prentice, Karl Pribram, and M. E. Bitterman, and from various associates at Princeton University, Kansas University, and the University of Michigan, where versions of this paper were presented.

ON THE NATURE OF PERCEPTION

Perception involves an act of categorization. Put in terms of the antecedent and subsequent conditions from which we make our inferences, we stimulate an organism with some appropriate input and he responds by referring the input to some class of things or events. "That is an orange," he states, or he presses a lever that he has been "tuned" to press when the object that he "perceives" is an orange. On the basis of certain defining or criterial attributes in the input, what are usually called cues although they should be called clues (35), there is a selective placing of the input in one category of identity rather than another. The category need not be elaborate: "a sound," "a touch," "a pain," are also examples of categorized inputs. The use of cues in inferring the categorial identity of a perceived object, most recently treated by Bruner, Goodnow, and Austin (9) and by Binder (4), is as much a feature of perception as the sensory stuff from which percepts are made. What is interesting about the nature of the inference from cue to identity in perception is that it is in no sense different from other kinds of categorial inferences based on defining attributes. "That thing is round and nubby in texture and orange in color and of such-and-such size—therefore an orange; let me now test its other properties to be sure." In terms of process, this course of events is no different from the more abstract task of looking at a number, determining that it is divisible only by itself and unity, and thereupon categorizing it in the class of prime numbers. So at the outset, it is evident

that one of the principal characteristics of perceiving is a characteristic of cognition generally. There is no reason to assume that the laws governing inferences of this kind are discontinuous as one moves from perceptual to more conceptual activities. In no sense need the process be conscious or deliberate. A theory of perception, we assert, needs a mechanism capable of inference and categorizing as much as one is needed in a theory of cognition.

Let it be plain that no claim is being made for the utter indistinguishability of perceptual and more conceptual inferences. In the first place, the former appear to be notably less docile or reversible than the latter. I may know that the Ames distorted room that looks so rectangular is indeed distorted, but unless conflicting cues are put into the situation, as in experiments to be discussed later, the room still looks rectangular. So too with such compelling illusions as the Miller-Lyer: in spite of knowledge to the contrary, the line with the extended arrowheads looks longer than the equal-length one with those inclined inward. But these differences, interesting in themselves, must not lead us to overlook the common feature of inference underlying so much of cognitive activity.

Is what we have said a denial of the classic doctrine of sense-data? Surely, one may argue (and Hebb [36] has done so effectively) that there must be certain forms of primitive organization within the perceptual field that make possible the differential use of cues in identity categorizing. Both logically and psychologically, the point is evident. Yet it seems to me foolish and unnecessary to assume that the sensory "stuff" on which higher order categorizations are based is, if you will, of a different sensory order than more evolved identities with which our perceptual world is normally peopled. To argue other-

wise is to be forced into the contradictions of Locke's distinction between primary and secondary qualities in perception. The rather bold assumption that we shall make at the outset is that all perceptual experience is necessarily the end product of a categorization process.

And this for two reasons. The first is that all perception is generic in the sense that whatever is perceived is placed in and achieves its "meaning" from a class of percepts with which it is grouped. To be sure, in each thing we encounter, there is an aspect of uniqueness, but the uniqueness inheres in deviation from the class to which an object is "assigned." Analytically, let it be noted, one may make a distinction, as Gestalt theorists have, between a pure stimulus process and the interaction of that stimulus process with an appropriate memory trace—the latter presumably resulting in a percept that has an identity. If indeed there is a "pure stimulus process," it is doubtful indeed that it is ever represented in perception bereft of identity characteristics. The phenomenon of a completely unplaceable object or event or "sensation"—even unplaceable with respect to modality—is sufficiently far from experience to be uncanny. Categorization of an object or event—placing it or giving it identity—can be likened to what in set theory is the placement of an element from a universe in a subset of that universe of items on the basis of such ordered dimensional pairs, triples, or n -tuples as man-woman, mesomorph-endomorph-ectomorph, or height to nearest inch. In short, when one specifies something more than that an element or object belongs to a universe, and that it belongs in a subset of the universe, one has categorized the element or object. The categorization can be as intersecting as "this is a quartz crystal goblet fashioned in Denmark," or as simple as "this is a

glassy thing." So long as an operation assigns an input to a subset, it is an act of categorization.

More serious, although it is "only a logical issue," is the question of how one could communicate or make public the presence of a nongeneric or completely unique perceptual experience. Neither language nor the tuning that one could give an organism to direct any other form of overt response could provide an account, save in generic or categorial terms. If perceptual experience is ever had raw, i.e., free of categorial identity, it is doomed to be a gem serene, locked in the silence of private experience.

Various writers, among them Gibson (26), Wallach (83), and Pratt (66), have proposed that we make a sharp distinction between the class of perceptual phenomena that have to do with the identity or object-meaning of things and the attributive or sensory world from which we derive our cues for inferring identities. Gibson, like Titchener (78) before him, urges a distinction between the visual field and the visual world, the former the world of attributive sense impressions, the latter of objects and things and events. Pratt urges that motivation and set and past experience may affect the things of the visual world but not the stuff of the visual field. And Wallach too reflects this ancient tradition of his Gestalt forebears by urging the distinction between a stimulus process pure and the stimulus process interacting with a memory trace of past experience with which it has made a neural contact on the basis of similarity. The former is the stuff of perception; the latter the finished percept. From shirtsleeves to shirtsleeves in three generations: we are back with the founding and founded content of the pre-Gestalt Gestalters. If one is to study the visual field freed of the things of the visual world, it becomes necessary—as Wallach implies—to free oneself of

the stimulus error: dealing with a percept not as an object or as a thing with identity, but as a magnitude or a brightness or a hue or a shape to be matched against a variable test patch.

If we have implied that categorizing is often a "silent" or unconscious process, that we do not experience a going-from-no-identity to an arrival-at-identity, but that the first hallmark of *any* perception is some form of identity, this does not free us of the responsibility of inquiring into the origin of categories. Certainly, Hebb (36) is correct in asserting like Immanuel Kant, that certain primitive unities or identities within perception must be innate or autochthonous and not learned. The primitive capacity to categorize "things" from "background" is very likely one such, and so too the capacity to distinguish events in one modality from those in others—although the phenomena of synesthesia would suggest that this is not so complete a juncture as it might seem; e.g., von Hornbostel (39). The sound of a buzz saw does rise and fall phenomenally as one switches room illumination on and off. The full repertory of innate categories—a favorite topic for philosophical debate in the 19th century—is a topic on which perhaps too much ink and too little empirical effort have been spilled. Motion; causation, intention, identity, equivalence, time, and space, it may be persuasively argued, are categories that must have some primitive counterpart in the neonate. And it may well be, as Piaget (65) implies, that certain primitive capacities to categorize in particular ways depend upon the existence of still more primitive ones. To identify something as having "caused" something else requires, first, the existence of an identity category such that the two things involved each may conserve identity in the process of "cause" producing "effect." Primitive or unlearned categories—a

matter of much concern to such students of instinctive behavior as Lashley (51) and Tinbergen (77)—remain to be explicated. In what follows, we shall rather cavalierly take them for granted. As to the development of more elaborated categories in terms of which objects are placed or identified, it involves the process of learning how to isolate, weigh, and use criterial attribute values, or cues for grouping objects in equivalence classes. It is only as mysterious, but no more so, than the learning of any differential discrimination, and we shall have occasion to revisit the problem later.

A second feature of perception, beyond its seemingly categorial and inferential nature, is that it can be described as varyingly veridical. This is what has classically been called the "representative function" of perception: what is perceived is somehow a representation of the external world—a metaphysical hodgepodge of a statement but one which we somehow manage to understand in spite of its confusion. We have long since given up simulacral theories of representation. What we generally mean when we speak of representation or veridicality is that perception is predictive in varying degrees. That is to say, the object that we *see* can also be *felt* and *smelled* and there will somehow be a match or a congruity between what we see, feel, and smell. Or, to paraphrase a younger Bertrand Russell, what we see will turn out to be the same thing should we take a "closer look" at it. Or, in still different terms, the categorial placement of the object leads to appropriate consequences in terms of later behavior directed toward the perceived object: it appears as an apple, and indeed it keeps the doctor away if consumed once a day.

Let it be said that philosophers, and notably the pragmatist C. S. Peirce, have been urging such a view for more

years than psychologists have taken their urgings seriously. The meaning of a proposition, as Peirce noted in his famous essay on the pragmatic theory of meaning (63), is the set of hypothetical statements one can make about attributes or consequences related to that proposition. "Let us ask what we mean by calling a thing *hard*. Evidently, that it will not be scratched by many other substances" (White, (84)). The meaning of a thing, thus, is the placement of an object in a network of hypothetical inference concerning its other observable properties, its effects, and so on.

All of this suggests, does it not, that veridicality is not so much a matter of representation as it is a matter of what I shall call "model building." In learning to perceive, we are learning the relations that exist between the properties of objects and events that we encounter, learning appropriate categories and category systems, *learning to predict and to check what goes with what*. A simple example illustrates the point. I present for tachistoscopic recognition two nonsense words, one a 0-order approximation to English constructed according to Shannon's rules, the other a 4-order approximation: YRULPZOC and VERNALIT. At 500 milliseconds of exposure, one perceives correctly and in their proper place about 48 per cent of the letters in 0-order words, and about 93 per cent of the letters in 4-order words. In terms of the amount of information transmitted by these letter arrays, i.e., correcting them for redundancy, the subject is actually receiving the same informational input. The difference in reportable perception is a function of the fact that the individual has learned the transitional probability model of what goes with what in English writing. We say that perception in one case is more "veridical" than in the other—the difference between 93 per

cent correct as contrasted with 48 per cent. What we mean is that the model of English with which the individual is working corresponds to the actual events that occur in English, and that if the stimulus input does not conform to the model, the resulting perception will be less veridical. Now let us drop the image of the model and adopt a more sensible terminology. Perceiving accurately under substandard conditions consists in being able to refer stimulus inputs to appropriate coding systems; where the information is fragmentary, one reads the missing properties of the stimulus input from the code to which part of the input has been referred. If the coding system applied does not match the input, what we read off from the coding system will lead to error and nonveridical perception. I would propose that perceptual learning consists not of making finer and finer discriminations as the Gibsons (27) would have us believe, but that it consists rather in the learning of appropriate modes of coding the environment in terms of its object character, connectedness, or redundancy, and then in allocating stimulus inputs to appropriate categorial coding systems.

The reader will properly ask, as Prentice (67) has, whether the notion of perceptual representation set forth here is appropriate to anything other than situations where the nature of the percept is not "clear"—perceptual representation under peripheral viewing conditions, in tachistoscopes, under extreme fatigue. If I am given a very good look at an object, under full illumination and with all the viewing time necessary, and end by calling it an orange, is this a different process from one in which the same object is flashed for a millisecond or two on the periphery of my retina with poor illumination? In the first and quite rare case the cues permitting the identification of the object are super-

abundant and the inferential mechanism operates with high probability relationships between cues and identities. In the latter, it is less so. The difference is of degree. What I am trying to say is that under *any* conditions of perception, what is achieved by the perceiver is the categorization of an object or sensory event in terms of more or less abundant and reliable cues. Representation consists of knowing how to utilize cues with reference to a system of categories. It also depends upon the creation of a system of categories-in-representation that fit the nature of the world in which the person must live. In fine, adequate perceptual representation involves the learning of appropriate categories, the learning of cues useful in placing objects appropriately in such systems of categories, and the learning of what objects are likely to occur in the environment, a matter to which we will turn later.

We have neglected one important feature of perceptual representation in our discussion: representation in perception of the space-time-intensity conditions of the external world. Perceptual magnitudes correspond in some degree to the metrical properties of the physical world that we infer from the nature of our perception. That is to say, when one line *looks* longer than another, it is likely to *be* longer as measured by the ruler. There are constant errors and sampling errors in such sensory representation, but on the whole there is enough isomorphism between perceiving without aids (psychology) and perceiving with aids (physics) to make the matter perennially interesting.

Is this form of representation subject to the kinds of considerations we have been passing in review? Does it depend upon categorizing activities and upon the construction of an adequate system of categories against which stimulus inputs can be matched? There is prob-

ably one condition where perceptual acts are relatively free of such influences, and that is in the task of discriminating simultaneously presented stimuli as alike or different—provided we do not count the “tuning of the organism” that leads one to base his judgment on one rather than another feature of the two stimuli. Ask the person to deal with one stimulus at a time, to array it in terms of some magnitude scale, and immediately one is back in the familiar territory of inferential categorizing. Prentice, in his able defense of formalism in the study of perception (67), seems to assume that there is a special status attached to perceptual research that limits the set of the observer to simple binary decisions of “like” and “different” or “present” and “absent,” and to research that also provides the subject with optimal stimulus conditions, and Graham (31) has recently expressed the credo that no perceptual laws will be proper or pure laws unless we reduce perceptual experimentation to the kinds of operations used in the method of constant stimuli.

There was at one time a justification for such a claim on the grounds that such is the best strategy for getting at the sensory-physiological processes that underlie perception. As we shall see in a later section, current work in neurophysiology brings this contention into serious doubt. In any case, the point must be made that many of the most interesting phenomena in sensory perception are precisely those that have been uncovered by departing from the rigid purism of the method of constants. I have in mind such pioneering studies as those of Stevens on sensory scales, where the organism is treated as an instrument whose sensory categorizations and scalar orderings are the specific object of study (74). Add to this the advances made by Helson on adaptation level (37) and by Volkman on the anchoring of sensory scales (82)—both

using the “sloppy” method of single stimuli—and one realizes that the nature of representation in perception of magnitudes is very much subject to categorizing processes, and to perceptual readiness as this is affected by subjective estimates of the likelihood of occurrence of sensory events of different magnitudes. Indeed, Helson’s law of adaptation level states that the subjective magnitude of a singly presented stimulus depends upon the weighted geometric mean of the series of stimuli that the subject has worked with, and the ingenious experiments of Donald Brown (7) have indicated that this adaptation level is influenced only by those stimuli that the subject considers to be within the category of objects being considered. Ask the subject to move a weight from one side of the table to the other with the excuse that it is cluttering up the table, and the weight does not serve as an anchor to the series, although it will show a discernible effect if it is directly included in the series being judged. In short, the category systems that are utilized in arraying magnitudes are also affected by the requirement of matching one’s model of the world to the actual events that are occurring—even if the categories be no more complicated than “heavy,” “medium,” and “light.”

The recent work of Stevens (75) on “the direct estimation of sensory magnitudes” highlights the manner in which veridicality in sensory judgment depends upon the prior learning of an adequate category set in terms of which sensory input may be ordered. Subjects are presented a standard tone of 1000 cps at 80 db. sound-pressure-level and are told that the value of this loudness is 10. Nine variable loudnesses all of the 1000 cps are then presented, varying 70 db. on either side of the standard, each one at a time being paired with the standard. “If the standard is called 10, what would you call

the variable? Use whatever numbers seem to you appropriate—fractions, decimals, or whole numbers." If one then compares the categorial judgments made with the sound pressure level of the various tones presented, using a log-log plot (log of the magnitude estimation against log of sound-pressure-level), the resulting function is a straight line, described by the empirical formula

$$L = kI^{0.3},$$

where L is loudness and I intensity. In short, categorial sorting of sensory magnitudes provides one with a mapping or representation of physical intensity. There are, to be sure, many problems connected with such a procedure, but the point remains: the magnitude categories in terms of which we scale sensory events represent a good fit to the physical characteristics of the world. Call this "veridicality" if you wish—although I do not see what is gained thereby; yet whatever one calls it, one must not lose sight of the fact that the judgments made are predictive of other features of the sensory inputs. Given the empirical conversion formula, one can predict from categorial judgment to physical meter readings.

To summarize, we have proposed that perception is a process of categorization in which organisms move inferentially from cues to categorial identity and that in many cases, as Helmholtz long ago suggested, the process is a silent one. If you will, the inference is often an "unconscious" one. Moreover, the results of such categorizations are representational in nature: they represent with varying degrees of predictive veridicality the nature of the physical world in which the organism operates. By predictive veridicality I mean simply that perceptual categorization of an object or event permits one to "go beyond" the properties of the object or event perceived to a prediction of other properties

of the object not yet tested. The more adequate the category systems constructed for coding environmental events in this way, the greater the predictive veridicality that results.

Doubtless, the reader will think of any number of examples of perceptual phenomena not covered by the simple picture we have drawn. Yet a great many of the classic phenomena are covered—psychophysical judgment, constancy, perceptual identification, perceptual learning, and so on. This will become clearer in the following sections. What must now be dealt with are the phenomena having to do with selectivity: attention, set, and the like.

CUE UTILIZATION AND CATEGORY ACCESSIBILITY

A fruitful way of thinking of the nature of perceptual readiness is in terms of the accessibility of categories for use in coding or identifying environmental events. Accessibility is a heuristic concept, and it may be defined in terms of a set of measures. Conceive of a person who is perceptually ready to encounter a certain object, an apple let us say. *How* he happens to be in this state we shall consider later. We measure the accessibility of the category "apples" by the amount of stimulus input of a certain pattern necessary to evoke the perceptual response "there is an apple," or some other standardized response. We can state the "minimum" input required for such categorization by having our observer operate with two response categories, "yes" and "no," with the likelihood of occurrence of apples and non-apples at 50:50, or by using any other definition of "maximum readiness" that one wishes to employ. The greater the accessibility of a category, (a) the less the input necessary for categorization to occur in terms of this category, (b) the wider the range of input characteristics that will be "accepted" as fitting

the category in question, (c) the more likely that categories that provide a better or equally good fit for the input will be masked. To put it in more ordinary language: apples will be more easily and swiftly recognized, a wider range of things will be identified or misidentified as apples, and in consequence the correct or best fitting identity of these other inputs will be masked. This is what is intended by accessibility.

Obviously, categories are not isolated. One has a category "apples," to be sure, but it is imbedded by past learning in a network of categories: "An apple a day keeps the doctor away" is one such category system. So too, are "apples are fruits" and other placements of an object in a general classification scheme. Predictive systems are of the same order: e.g., "The apple will rot if not refrigerated." We have spoken of these systems before as the "meaning" of an object. We mention them again here to indicate that though we speak analytically of separate or isolated categories as being accessible to inputs, it is quite obvious that category systems vary in accessibility as a whole.

It follows from what has just been said that the most appropriate pattern of readiness at any given moment would be that one which would lead on the average to the most "veridical" guess about the nature of the world around one at the moment—best guess here being construed, of course, as a response in the absence of the necessary stimulus input. And it follows from this that the most ready perceiver would then have the best chances of estimating situations most adequately and planning accordingly. It is in this general sense that the ready perceiver who can proceed with fairly minimal inputs is also in a position to use his cognitive readiness not only for perceiving what is before him but in foreseeing what is likely to

be before him. We shall return to this point shortly.

We must turn now to the question of cue utilization, the "strategies" in terms of which inferences are made (by the nervous system, of course) from cue to category and thence to other cues. I prefer to use the term strategy for several reasons. Perceiving, since it involves inference, rests upon a decision process, as Brunswik (17), Tanner and Swets (76) and others have pointed out. Even in the simplest threshold-measurement test, the subject has the task of deciding whether what he is seeing or hearing is noise only or signal-plus-noise. Given a set of cues, however presented, my nervous system must "decide" whether the thing is an airplane or a sea gull, a red or a green, or what not.

There appears, moreover, to be a sequence of such decisions involved in categorizing an object or event. A common-sense example will make this clear. I look across to the mantelpiece opposite my desk and see a rectangular object lying on it. If I continue this pursuit, subsequent decisions are to be made: is it the block of plastic I purchased for some apparatus or is it a book? In the dim light it can be either. I remember that the plastic is downstairs in one of the experimental rooms: the object "is" a book now, and I search for further cues on its dark red surface. I see what I think is some gold: it is a McGraw-Hill book, probably G. A. Miller's *Language and Communication* that I had been using late this afternoon. If you will, the process is a "bracketing" one, a gradual narrowing of the category placement of the object.

Let us attempt to analyze the various stages in such a decision sequence.

a. Primitive categorization. Before any more elaborate inferential activity can occur, there must be a first, "silent"

process that results in the perceptual isolation of an object or an event with certain characteristic qualities. Whether this is an innate process or one depending upon the prior construction of a cell-assembly, in the manner of Hebb (36), need not concern us. What is required simply is that an environmental event has been perceptually isolated and that the event is marked by certain spatio-temporal-qualitative characteristics. The event may have no more "meaning" than that it is an "object," a "sound," or a "movement."

b. Cue search. In highly practiced cases or in cases of high cue-category probability linkage, a second process of more precise placement based on additional cues may be equally silent or "unconscious." An object is seen with phenomenal immediacy as a "book" or an "ash tray." In such instances there is usually a good fit between the specifications of a category and the nature of the cues impinging on the organism—although "fit" and "probability of linkage" may stand in a vicarious relation to each other. Where the fit to accessible categories is not precise, or when the linkage between cue and category is low in probability in the past experience of the organism, the conscious experience of cue searching occurs. "What is that thing?" Here, one is scanning the environment for data in order to find cues that permit a more precise placement of the object. Under these circumstances, the organism is "open" to maximum stimulation, in a manner described below.

c. Confirmation check. When a tentative categorization has occurred, following cue search, cue search changes. The "openness" to stimulation decreases sharply in the sense that now, a tentative placement of identity having occurred, the search is narrowed for additional confirmatory cues to check this

placement. It is this feature of perceptual identification that Woodworth (85) in his paper on the "Reinforcement of Perception" speaks of as "trial-and-check." We shall speak of a selective gating process coming into operation in this stage, having the effect of reducing the effective input of stimulation not relevant to the confirmatory process.

d. Confirmation completion. The last stage in the process of perceptual identification is a completion, marked by termination of cue searching. It is characteristic of this state that openness to additional cues is drastically reduced, and incongruent cues are either normalized or "gated out." Experiments on the perception of incongruity (14), error (69), and the like (15), suggest that once an object has been categorized in a high-probability, good-fit category, the threshold for recognizing cues contrary to this categorization increases by almost an order of magnitude.

The question of fit between cue and category specification brings us to the key problem of the nature of categories. By a category we mean a rule for classifying objects as equivalent. The rule specifies the following about the instances that are to be comprised in the category.

a. The properties or *critical attribute values* required of an instance to be coded in a given class.

b. The manner in which such attribute values are to be combined in making an inference from properties to category membership: whether conjunctively (e.g., a_i and b_i), relationally (e.g., a_i bears a certain *relation* to b_i), or disjunctively (e.g., a_i or b_i).

c. The weight assigned various properties in making an inference from properties to category membership.

d. The acceptance limits within which properties must fall to be critical. That

is to say, from what range of attribute values may a_i, b_i, \dots, k_i be drawn.

When we speak of rules, again it should be made clear that "conscious rules" are not intended. These are the rules that govern the operation of a categorizing mechanism.

The likelihood that a sensory input will be categorized in terms of a given category is not only a matter of fit between sensory input and category specifications. It depends also on the accessibility of a category. To put the matter in an oversimplified way, given a sensory input with equally good fit to two nonoverlapping categories, the more accessible of the two categories would "capture" the input. It is in this sense that mention was earlier made about the vicarious relationship between fit and accessibility.

We have already noted that the accessibility of categories reflects the learned probabilities of occurrence of events in the person's world. The more frequently in a given context instances of a given category occur, the greater the accessibility of the category. Operationally, this means that less stimulus input will be required for the instance or event to be categorized in terms of a frequently used category. In general, the type of probability we are referring to is not absolute probability of occurrence, where each event that occurs is independent of each other. Such independence is rare in the environment. Rather, the principal form of probability learning affecting category accessibility is the learning of contingent or transitional probabilities—the redundant structure of the environment. That either the absolute or the contingent probability of events makes a crucial difference in determining ease of perceptual identification is readily supported by research findings: in the former case by studies like those of Howes (40) and Solomon

and Postman (72), and in the latter by the work of Miller, Heise, and Lichten (62) and Miller, Bruner, and Postman (61).

But the organism to operate adequately must not only be ready for likely events in the environment, the better to represent them, and in order to perceive them quickly and without undue cognitive strain: it must also be able to search out unlikely objects and events essential to its maintenance and the pursuit of its enterprises. If I am walking the streets of a strange city and find myself hungry, I must be able to look for restaurants regardless of their likelihood of occurrence in the environment where I now find myself. In short, the accessibility of categories I employ for identifying the objects of the world around me must not only reflect the environmental probabilities of objects that fit these categories, but also reflect the search requirements imposed by my needs, my ongoing activities, my defenses, etc. And for effective search behavior to occur, the pattern of perceptual readiness during search must be realistic: tempered by what one is likely to find in one's perceptual world at that time and at that place as well as by what one seeks to find.

Let me summarize our considerations about the general properties of perception with a few propositions. The first is that *perception is a decision process*. Whatever the nature of the task set, the perceiver or his nervous system decides that a thing perceived is one thing and not another. A line is longer or shorter than a standard, a particular object is a snake and not a fallen branch, the incomplete word L*VE in the context MEN L*VE WOMEN is the word LOVE and not LIVE.

The second proposition is that *the decision process involves the utilization of discriminatory cues*, as do all decision processes. That is to say, the properties

of stimulus inputs make it possible to sort these inputs into categories of best fit.

Thirdly, *the cue utilization process involves the operation of inference.* Going from cue to an inference of identity is probably the most ubiquitous and primitive cognitive activity. The utilization of inference presupposes the learning of environmental probabilities and invariances relating cues to cues, and cues to behavioral consequences. Cue utilization involves various stages: a primitive step of isolating an object or event from the flux of environmental stimulation, stages of cue searching where the task is to find cues that can be fitted to available category specifications, a tentative categorization with more search for confirming cues, and final categorization, when cue searching is severely reduced.

Fourth, *a category may be regarded as a set of specifications* regarding what events will be grouped as equivalent—rules respecting the nature of criterial cues required, the manner of their combining, their inferential weight, and the acceptance limits of their variability.

Fifth, *categories vary in terms of their accessibility*, the readiness with which a stimulus input with given properties will be coded or identified in terms of a category. The relative accessibility of categories and systems of categories seems to depend upon two factors: the expectancies of the person with regard to the likelihood of events to be encountered in the environment; and the search requirements imposed on the organism by his needs and his ongoing enterprises. To use the functionalist's language, perceptual readiness or accessibility serves two functions: *to minimize the surprise value of the environment* by matching category accessibility to the probabilities of events in the world about one, and *to maximize the*

attainment of sought-after objects and events.

Veridical perception, so our sixth proposition would run, *consists of the coding of stimulus inputs in appropriate categories* such that one may go from cue to categorial identification, and thence to the correct inference or prediction of other properties of the object so categorized. Thus, veridical perception requires the learning of categories and category systems appropriate to the events and objects with which the person has commerce in the physical world. When we speak of the representative function of perception, we speak of the adequacy of the categorizing system of the individual in permitting him to infer the nature of events and to go beyond them to the correct prediction of other events.

Seventh, *under less than optimal conditions, perception will be veridical in the degree to which the accessibility of categorizing systems reflects the likelihood of occurrence of the events that the person will encounter.* Where accessibility of categories reflects environmental probabilities, the organism is in the position of requiring less stimulus input, less redundancy of cues for the appropriate categorization of objects. In like vein, nonveridical perception will be systematic rather than random in its error insofar as it reflects the inappropriate readiness of the perceiver. The more inappropriate the readiness, the greater the input or redundancy of cues required for appropriate categorization to occur—where "appropriate" means that an input is coded in the category that yields more adequate subsequent predictions.

MECHANISMS MEDIATING PERCEPTUAL READINESS

Having considered some of the most general characteristics of perceiving, particularly as these relate to the phenomena of perceptual readiness, we must

turn next to a consideration of the kinds of mechanisms that mediate such phenomena. Four general types of mechanisms will be proposed: *grouping and integration, access ordering, match-mismatch signaling, and gating*. They will be described in such a form that they may be considered as prototypes of neural mechanisms and, where possible, neurophysiological counterparts will be described briefly. Six years ago, Edward Tolman (79) proposed that the time was perhaps ripe for reconsidering the neural substrate of perception. Perhaps he was right, or perhaps even now the enterprise is somewhat premature. Yet, the body of perceptual data available makes it worth while to consider the kinds of mechanisms that will be required to deal with them. To use Hebb's engaging metaphor, it is worth while to build a bridge between neurophysiology and psychology provided we are anchored at both ends, even if the middle of the bridge is very shaky.

Grouping and Integration

It is with the neural basis of the categorizing process that Hebb's *Organization of Behavior* (36) is principally concerned. Little is served by recapitulating his proposals here, for the reader will be familiar with the concise account in Chapters 4 and 5 of that book, where the concepts of cell assembly and phase sequence are set forth with a clarity that permits one to distinguish what is neurophysiological fact and what speculation. In essence, Hebb's account attempts to provide an anatomical-physiological theory of how it is that we distinguish classes of events in the environment, and how we come to recognize new events as exemplars of the once established classes. The theory seeks also to provide a mechanism for integration of sorting activity over time: the formation of phase sequences for the conservation of superordinate classes

of events and superordinate sequences. Basically, it is an associational or an "enrichment" theory of perception at the neural level, requiring that established neural associations facilitate perception of events that have gone together before. The expectancies, the centrally induced facilitations that occur prior to the sensory process for which they are appropriate, are learned expectancies based on the existence of frequency integrators. These frequency integrators may be neuroanatomical in the form of synaptic knobs, or they may be any process that has the effect of making activity in one locus of the brain increase or decrease the likelihood of activity in another. To be sure, Hebb's theory depends upon some broad assumptions about convergence of firing from area 17 outward, about synchronization of impulses, and about the manner in which reverberatory circuits can carry organization until the much slower process of anatomical change can take place. But this is minor in comparison with the stimulation provided by facing squarely the question of how the known facts of categorization and superordination in perception *could* be represented in the light of present knowledge.

While it is difficult indeed to propose a plausible neural mediator to account for category formation and the development of elaborated categorial systems (e.g., our knowledge of the relations between classes of events in the physical world which we manipulate in everyday life), it is less difficult to specify what such mechanisms must account for in perceptual behavior.

At the level of the individual category or cell assembly, the phenomena of object identity must be accounted for. Moreover, identity conservation or object constancy requires explanation in terms common with the explanation of identity. Experiments by Piaget (65)

suggest that the capacity to maintain the phenomenal identity of an object undergoing change is the hard-won result of maturation-and-learning. In connection with the later discussion of gating processes, we shall have occasion to consider the manner in which, at different stages in cue utilization, the required fit between an input and a cell assembly changes.

Where integration is concerned, there must be a process capable of conserving a record of the likely transitions and contingencies of the environment. The moment-to-moment programming of perceptual readiness depends upon such integrations. In short, the relation between classes of events is conserved in such a way as to be subject to change by learning. Several things can be guessed about integration processes. It is unlikely that it is a simple autocorrelation device. Clearly, the conceptions of transitional probabilities that are established in dealing with sequences of events show biases that no self-respecting autocorrelation computer would be likely to operate with. One of these is a strong and early tendency to treat events as nonindependent of each other over time. In the absence of evidence, or even in the presence of contrary evidence, humans—as their behavior has been observed in choice tasks, e.g., Estes (23), Goodnow (29)—treat random sequences of events as though they were governed by dependent probabilities. The spate of research on two-choice decision behavior has made us quite sharply aware of this characteristic of cognitive functioning. The typical pattern is the gambler's fallacy or, more properly, the negative recency effect. Given two equiprobable events whose occurrences are random, the repetition of one event progressively leads to the expectancy of the other. As in the elegant experiments of Jarvik (44) and Goodnow (29), the probability that a person will

predict one of two events increases directly as a function of the number of repetitions of the other event. Such behavior persists over thousands of opportunities for testing, and it appears under a variety of testing conditions (9).

The second feature of sequential probability integration mechanisms is that, in establishing a conception of the probability with which events will occur, the typical human subject will bias his estimate in terms of desired or feared outcomes. As in the experiments of Marks (60) on children and of Irwin (41) on adults, the subjectively estimated probability of strongly desired events will be higher per previous encountered occurrence than the estimated likelihood of less desired events. Quite clearly, then, the establishment of estimates depends upon more than frequency integrations biased by assumptions of nonindependence. The "something more" is a motivational or personality process, and we shall have more to say about it in considering phenomena of so-called "perceptual sensitization" and "perceptual defense."

Access Ordering

The term "accessibility" has been used in preceding pages to denote the ease or speed with which a given stimulus input is coded in terms of a given category under varying conditions of instruction, past learning, motivation, etc. It has been suggested, moreover, that two general sets of conditions affect accessibility: subjective probability estimates of the likelihood of a given event, and certain kinds of search sets induced by needs and by a variety of other factors.

Let us consider a few relevant facts about perception. The first of these is that the threshold of recognition for stimuli presented by visual, auditory, or other means is not only a function of

the time, intensity, or "fittingness" of the stimulus input, but also varies massively as a function of the number of alternatives for which the perceiver is set. The size of the expected array, to say it another way, increases the identification threshold for any item in the array. Typical examples of this general finding are contained in papers by Miller, Heise, and Lichten (62) and by Bruner, Miller, and Zimmerman (10). The actual shape of the function need not concern us, save that it is quite clear that it is not what one would expect from a simple binary system with a fixed channel capacity. What we are saying holds, of course, only for the case where the perceiver has learned that all the items in the expected array are (a) equiprobable and (b) independent, one of the other, in order of appearance.

The first hunch we may propose, then, about access-ordering mechanisms is that degree of accessibility of coding categories to stimulus inputs is related to regulation of the number of preactivated cell assemblies that are operative at the time of input. In an earlier paper (8), discussing factors that strengthen an hypothesis in the sense of making it more easily confirmable, I proposed that one of the major determinants of such strength was monopoly: where one and only one hypothesis is operative with no competing alternatives, it tends to be more readily confirmable. It is the same general point that is being made here. Accessibility, then, must have something to do with the resolution of competing alternatives.

As between two arrays of expected alternatives, each of the same size, we may distinguish between them in terms of the bias that exists in terms of expected likelihood of occurrence of each alternative. If one could characterize the expected alternatives in terms of probability values, one could conceive of the array ranging in values from a figure approaching 1.0 at

one extreme, to another approaching 0.0 at the other. The findings with respect to perceptual readiness for the alternatives represented in such an array are well known. For a constant-sized array, the greater the estimated likelihood of occurrence of an alternative, the more readily will the alternative be perceived or identified. This is known to be true for large arrays, such as the ensemble of known words in the English language, whose likelihood may be roughly judged by their frequency of occurrence in printed English (e.g., 40). It is not altogether clear that it is the case for arrays of expected alternatives that are within the so-called span of attention—i.e., less than seven or eight alternatives. That the principle holds for middling arrays of about 20 items has been shown by Solomon and Postman (72).

What is particularly interesting about change of accessibility, under conditions where estimates of the likelihood of occurrence of alternatives become biased, is that the biasing can be produced either by a gradual learning process akin to probability learning or by instruction. Thus, Bitterman and Kniffin (5), investigating recognition thresholds for taboo and neutral words, show that as the experiment progresses, there is a gradual lowering of threshold for the taboo words as the subject comes to expect their occurrence. Bruner and Postman (14) have similarly shown that repeated presentation of stimulus materials containing very low-probability incongruities leads to a marked decrease in threshold time required for recognizing the incongruous features. At the same time, both Cowen and Beier (20) and Postman and Crutchfield (70) have shown that if a subject is forewarned that taboo words are going to be presented, his threshold for them will tend to be lower than for neutral words, whereas it will be higher if no instruction is given. In short, preactivation of

cell assemblies—assuming for a moment that *degree of preactivation* is the mechanism that represents subjective estimates of likelihood of occurrence of an event—such preactivation can be produced by gradual learning or quantally by instruction. Moreover, biasing may be produced by the nature of the situation in which the perceiver is operating. A recent study by Bruner and Minturn (11) illustrates the point. Subjects are presented at brief exposure a broken capital B with a small separation between the vertical and the curved component of the letter so that it may be perceived as a B or as a 13. The manner in which it is reported is determined by whether the subject has previously been presented with letters or with numbers to recognize. In short, expectancy of one or the other context preactivates a related array of categories or cell-assemblies, not just a single, isolated one.

What the neural correlates of access ordering will look like is anybody's guess. Lashley (52) has remarked that, for all our searching, we have not located a specific memory trace—either in the form of a reverberatory circuit, a definite change in fiber size as proposed by J. Z. Young (88) and Eccles (21), a synaptic knob—in the manner of Lorente de No (57) or in any known form. To be sure, Penfield (64) has activated memories by punctate electrical stimulation of the cortex, but this is a long remove from a definition of the neural properties of the trace. For the time being, one does better to deal in terms of the formal properties that a trace system must exhibit than to rest one's psychological model on any neurophysiological or anatomical conception of the memory trace.

And, quite clearly, one of the formal properties of a trace system is that its elements vary in accessibility to stimulus input with the kinds of conditions we have considered. It is instructive to

note that when a theory of traces lacks this feature, it ceases to be useful in dealing with the wide range of perceptual categorizing phenomena of which we now have knowledge. Gestalt theory is a case in point. According to Köhler's view (48), a stimulus process "finds" its appropriate memory trace, resulting in identification of the stimulus process, on the basis of distinctive similarity between stimulus process and memory trace. The theory has been criticized, justly I think, for failing to specify the nature of this similarity save by saying that it is a neural isomorph of phenomenal similarity. But since similarity may be highly selective—two objects may be alike in color but differ in dozens of other respects—there is obviously some *tertium quid* that determines the basis of similarity. More serious still is the inability of such a theory to deal with the increased likelihood of categorization in terms of particular traces as a function of changes in search set or subjective likelihood estimates. The Bruner-Minturn results would require that, as between two traces with which a stimulus process may make contact, each equally "similar" to the stimulus, the stimulus process will make contact with the one having a higher probability of being matched by environmental events. This is interesting, but it is far from the spirit of Gestalt theory.

Match-Mismatch Processes

One may readily conceive of and, indeed, build an apparatus that will accept or reject inputs on the basis of whether or not they fulfill certain specifications. Selfridge (71) has constructed a machine to read letters, Fry (24) has one that will discriminate various phonemes, and Uttley (80) has constructed one that, like Tinbergen's graylag geese, will recognize the flying silhouette of a predator hawk. All such machines have

in common that they require a match between a stimulus input and various specifications required by the sorting mechanism of the machine.

In the examples just given, there is no consequence generated by whether a given input fulfills the specifications required by the identifying machine. It fits or it doesn't fit. But now let us build in two other features. The first is that the machine emit a signal to indicate how closely any given input comes to fulfilling the specifications required: either by indicating how many attributes the object has in common with the specifications, or by indicating how far off the mark on any given attribute dimension a given input is. The second is that the machine do something on the basis of these signals: to increase sensitivity if an object is within a given distance of specifications for a closer look, or to decrease it if the object is further than a certain amount from specifications, or to stop registering further if the input fits.

In short, one can imagine a nervous system that emits all-or-none match-mismatch signals or graded match-mismatch signals, and one can also imagine that these signals could then feed into an effector system to regulate activity relevant to continuing search behavior for a fitting object, or to regulate other forms of activity. MacKay (59) has recently proposed such a model.

We must return for a moment to an earlier discussion. In the discussion of cue utilization, a distinction was made between three phases of "openness" in cue search. The first was one in which a given input was being scanned for its properties so as to place it in one of a relatively large set of possible alternative categories. Here one would register on as many features of an object as possible. In a second stage, the input has been tentatively placed, and the search is limited to confirming or in-

firming criterial cues. Finally, with more definite placement, cue search is suspended and deviations from specification may even be "normalized." It is for the regulation of such patterns of search or cue utilization that some mechanism such as match-mismatch signaling is postulated.

Let it be said that while match-mismatch signaling-effector systems are readily conceivable and readily constructed, there is no knowledge available as to how a system like the nervous system might effect such a process. That there is feedback all over the system is quite apparent from its detailed anatomy, and this is the process out of which a larger-scale system such as we have described would be constructed.

Gating Processes

The picture thus far presented is of a conceptual nervous system with a massive afferent intake that manages somehow to sort inputs into appropriate assemblies of varying accessibility. It seems unlikely that this is the nature of the nervous system, that there should be no gating or monitoring of stimulus input short of what occurs at higher centers. It is with this more peripheral form of screening of inputs that we shall now be concerned.

It has long been known that the concept of the "adequate stimulus" could not simply be defined as a change in environmental energy sufficient to stimulate a receptor. For quite evidently, a stimulus could be peripherally adequate in this sense and not be "centrally" adequate at all, either in eliciting electrical activity in the cortex or in producing a verbal report of a change in experience by the subject. Indeed, the very nature of such complex receptor surfaces as the retina argues against such a simple notion of "adequate stimulus." For the reactivity of even a retinal cell at the fovea seems to be "gated" by the state

of stimulation of neighboring cells. Thus, if cells A, B, and C lie next each other in that order in a row, stimulation of B suppresses the sensitivity of C. If A now be stimulated, B is suppressed and C is released or heightened in sensitivity. So even at the level of the first synapse of a sensory system, there is mediation *outward* or gating from internuncial to receptor cells that programs the nature of the input that can come into the sensory system. And to be sure, there are many phenomena in perception itself that speak for this same kind of gating. When we are fixated upon the vase in the Rubin reversible figure, the background recedes, is less surfacic, and in general seems to provide a generally less centrally adequate form of sensory input. So too with the studies of Yokoyama (87) and Chapman (19) where subjects, set to report on one of several attributes of briefly presented stimuli, accomplished their selective task with a loss of ability to discriminate on the attributes for which they had not been set. We shall propose that such phenomena are very likely mediated by a gating process which "filters" input before ever it reaches the cortex.

There is now a growing body of neurophysiological evidence that part of this screening process is relegated to peripheral levels of the nervous system—even as far out as the second synapse of specialized sensory systems. In an earlier paper I used the rather fanciful phrase that "perception acts sometimes as a welcoming committee and sometimes as a screening committee." It now appears that both these committees are closer to the entrance port than previously conceived.

Consider first the evidence of Kuffler and Hunt (50) on so simple a "reflex" as the stretch reflex of the biceps femoris muscle of the cat in an isolated spinal nerve-muscle preparation. Recall a little anatomy first. Muscle tissue con-

tains special cells called spindles that are receptors in function, discharging with contraction or stretch of the muscle in which they are imbedded. The muscle itself is innervated by an efferent nerve trunk emerging from the ventral horn of the spinal cord and, in turn, an afferent nerve travels to the dorsal root of the spinal cord. According to the classical law of Bell and Magendie, the ventral root of the spinal cord carries efferent-motor impulses down to the muscles, while the dorsal root carries sensory impulses up to the cord. Now, it has been known for a long time that the presumed efferent nerve going to muscles carries fibers of large and of small diameter. A quarter-century ago Eccles and Sherrington showed that the ventral nerve branch supplying the biceps femoris of the cat shows a "striking division of the fibers into two diameter groups" (49), one group centering around 5μ in diameter, the other around 15 or 16μ . The large fibers are, of course, fast conductors, the small ones slow. Leksell (55) has shown that stimulation of the slow-conducting smaller fibers did not cause detectable contractions or propagated muscle impulses. When the larger and fast-conducting fibers are stimulated, the usual motor-unit twitch occurred. Kuffler and Hunt (50) state that, in the lumbosacral outflow, about $\frac{2}{3}$ of the fibers are of the large-diameter, fast-conduction type; the other third are of the small type that in mammals are "ineffective in directly setting up significant muscular contraction." There has been much speculation about what these fibers are there for, and the answer is now fairly clear. It is revolutionary in its implications and brings deeply into question both the classical Bell-Magendie law and the simplistic notion of the reflex arc on which so much of American learning theory is based.

It is this. The small fibers of the

presumably motor trunk go to the spindle cells and the activity in these fibers serve to modulate or gate the receptivity of these specialized sensory endings. For example, if the small-diameter fibers are firing into the muscle spindle it may speed up the amount of firing from this cell into the afferent nerve that is produced by a given amount of stretch tension on the muscle. We need not go into detail here. It suffices to note that the state of presumed motor discharge does not simply innervate the muscle; it also regulates the amount and kind of kinesthetic sensory discharge that the sensory cells in the muscle will send back to the central nervous system. Instead of thinking of a stimulus-response reflex arc, it becomes necessary even at this peripheral level to think of the efferent portion of the arc acting back on sensory receptors to change the nature of the stimulus that can get through.

Two additional pieces of evidence on gating mechanisms at higher levels of integration may be cited. Where vision is concerned, Granit (32) has recently shown that pupillary changes produced by the ciliary muscle of the eye create changes in the pattern of firing of the retina: changes in muscular state working its way back through the nervous system into the visual system and back outward to the retina. There is also evidence of gating working from the visual system backward in the opposite direction: during binocular rivalry, the nondominant eye shows a less sensitive pupillary reflex than the dominant eye.

Finally, we may cite the recent evidence of Hernandez-Péon, Scherrer, and Juvet (38) working in Magoun's laboratory, work confirmed by analogous findings of Golambos, Sheatz, and Vernier (28) at the Walter Reed Hospital. If one stimulates the cat with auditory clicks, it is possible to record an evoked spike potential from the cochlear nucleus. Repetition of the clicks leads

to a gradual diminution of the evoked potential, as if the organism were adapting. It is quite extraordinary that such adaptation should be registered as far out peripherally as the cochlear nucleus, which is, after all, only the second synapse of the VIIIth nerve. Now, if the clicks are previously used as conditioned stimuli signaling shock, the diminution of the evoked potential no longer occurs upon repetition of the clicks. Evidence that the response from the brain is not being produced by the muscular activity produced by the click as a conditioned stimulus is provided by the fact that the same kind of effects are obtained from cats with temporarily induced muscular paralysis. Further, if one take a cat whose cochlear nucleus is still firing upon click stimulation and introduce a mouse into its visual field, the clicks no longer evoke a spike potential. A fish odor or a shock to the paw has the same effect of inhibiting spike potentials at the cochlear nucleus, if these distracting stimuli occur concurrently with the click. "Distraction" or "shifting of attention" appears to work its way outward to the cochlear nucleus.²

Perhaps the foregoing account has been needlessly detailed on the side of neurophysiology. Yet, the interesting implications of the findings for perceptual theory make such an excursion worth while. That the nervous system accomplishes something like gating is quite clear, even without the neurophysiological evidence. The data of behavior are full of examples, and the phenomena of attention require some such mechanism to be explained. Indeed, it is quite

² Since the above was written, evidence has been presented by Golambos indicating that efferently controlled inhibition operates as far out to the periphery as the hair cells of the organ of Corti and fibers carrying such inhibitory impulses have been traced as far centrally as the superior olivary nucleus—not very far, but a start.

clear that the nervous system must be capable of more selective gating than physiology has yet been able to discover. That is to say, there must be a filter somewhere in the cat's nervous system that will "pass" the squeak of the mouse in the Hernandez-Péon experiment but not the cough of the experimenter. And it is to this problem that we turn now.

I would propose that one of the mechanisms operative in regulating search behavior is some sort of gating or filtering system. In the preceding section, it was proposed that the "openness" of the first stage of cue utilization, the "selectivity" of the second stage, and the "closedness" of the third stage were probably regulated by a match-mismatch mechanism. What may be proposed here is that the degree of "openness" or "closedness" to sensory input during different phases of cue utilization is likely effected by the kind of gating processes we have been considering. How these work in intimate detail is far from known, yet the work of the last years in neurophysiology suggests that we are drawing closer to an answer.

Having considered some general properties of perception and some possible mechanisms underlying these, we turn now to some selected problems in perception better to explore the implications of what has thus far been proposed.

ON FAILURE OF READINESS

From the foregoing discussion, it is clear that veridical perception under viewing or listening conditions that are less than ideal depends upon a state of perceptual readiness that matches the probability of occurrence of events in the world of the perceiver. This is true, of course, only in a statistical sense. What is most likely to occur is not necessarily what will occur, and the perceiver whose readiness is well matched to the likelihoods of his environment

may be duped. In Farquhar's handsome seventeenth-century phrase: "I cou'd be mighty foolish, and fancy myself mighty witty; reason still keeps its Throne—but it nods a little, that's all." The only assurance against the nodding of reason or probability, under the circumstances, is the maintenance of a flexibility of readiness: an ability to permit one's hypotheses about what it is that is to be perceptually encountered to be easily infirmed by sensory input. But this is a topic for later.

There appear to be two antidotes to nonveridical perception, two ways of overcoming inappropriate perceptual readinesses. The one is a re-education of the misperceiver's expectancies concerning the events he is to encounter. The other is the "constant close look." If the re-education succeeds in producing a better match between internal expectancies and external event-probabilities, the danger of misperception under hurried or substandard conditions of perceiving is lessened. But the matter of re-educating perceptual expectancies is complex. For where consequences are grave, expectancy concerning what may be encountered does not change easily, even with continued opportunity to test the environment. In this concluding section we shall consider some of the factors that contribute to states of perceptual "unreadiness" that either fail to match the likelihood of environmental events or fail to reflect the requirements of adjustment or both.

Before turning to this task, a word is in order about the "constant close look" as an antidote to inappropriate perceptual readiness. There is for every category of objects that has been established in the organism a stimulus input of sufficient duration and cue redundancy such that, if the stimulus input fits the specifications of the category, it will eventually be correctly perceived as an exemplar of that category. With enough

time and enough testing of defining cues, such "best fit" perceiving can be accomplished for most but not all classes of environmental events with which the person has contact. There are some objects whose cues to identity are sufficiently equivocal so that no such resolution can be achieved, and these are mostly in the sphere of so-called interpersonal perception: perceiving the states of other people, their characteristics, intentions, etc., on the basis of external signs. And since this is the domain where misperception can have the most chronic if not the most acute consequences, it is doubtful whether a therapeutic regimen of "close looking" will aid the misperceiver much in dealing with more complex cue patterns. But the greatest difficulty rests in the fact that the cost of close looks is generally too high under the conditions of speed, risk, and limited capacity imposed upon organisms by their environment or their constitutions. The ability to use minimal cues quickly in categorizing the events of the environment is what gives the organism its lead time in adjusting to events. Pause and close inspection inevitably cut down on this precious interval for adjustment.

Inappropriate Categories

Perhaps the most primitive form of perceptual unreadiness for dealing with a particular environment is the case in which the perceiver has a set of categories that are inappropriate for adequate prediction of his environment. A frequently cited example of such a case is Bartlett's account (3) of the African visitors in London who perceived the London bobbies as especially friendly because they frequently raised their right hand, palm forward, to the approaching traffic. The cue-category inference was, of course, incorrect, and they should have identified the cue as a signal for stopping traffic. The example, however,

is not particularly interesting because it is a transient phenomenon, soon corrected by instruction.

A more interesting example, because it is far less tractable, is provided by second-language learning and the learning of a new phonemic system. Why is it, we may ask, that a person can learn the structure of a new language, its form classes, morphemes, lexemes, and so on, but still retain a "foreign accent" which he cannot, after a while, distinguish from the speech flow of native speakers around him? And why is it that a person learning a new language can follow the speech of a person with his own kind of foreign accent more readily than he can follow a native speaker? The answer lies, I think, in the phenomenon of postcategorization sensory gating: once an utterance has been "understood" or decoded in appropriate categories, on the basis of some of the diacritica of the speech flow, the remaining features are assimilated or normalized or screened out. The phonemic categories that are used, moreover, are modifications of those in the first language of the speaker. Normalization is in the direction of these first-language phonemic categories. It is only by a special effort that, after having achieved adequate comprehension of the second language, one can remain sensorially "open" enough to register on the deviation between his own phonemic pattern and that of native speakers. And since there is common categorization of the "meaning" of utterances by the native speaker and the fluent foreigner, there is no built-in incentive for the foreigner to maintain a cognitively strainful regimen of attending further to speech sounds.

Lenneberg (56) has recently shown the difficulties involved in learning new modes of categorizing such continua as chromatic colors. He taught subjects various nonsense languages, explaining

to them that the words were Hopi names for colors and that their task was to learn what colors they stood for. His stimulus materials were graded Munsell colors going in a circle from *brown*, through *green*, through *blue*, through *pink*, and then back to *brown*. A standardizing group was used to find the frequency distribution of color naming over the circle when the English color names mentioned above were used. Experimental groups, six in number, were then run, each being exposed to the use of the nonsense color names "as these are used by the Hopi." Then they were tested on their usage of the names. A first group was taught the nonsense words with exact correspondence to the usage found for the standardizing group on *brown*, *blue*, *green*, and *pink*. The other groups were given distorted usage training—distorted from English usage. The distortions were both in the slopes of the frequency of usage and in the points on the color continua where the highest usage frequencies fell. That is to say, the mode of a distribution in some cases would fall at a color which in English had no specific name, or fall between two English categories.

The principal results of the experiment are these. If the reference and probability relationship is the same for a nonsense language as it is for English, relearning is very rapid. The slightest deviation from this correspondence increases difficulty of learning quite markedly. It is disturbing either to shift the center of the categories on the color continuum or to change the shape of the frequency-of-calling functions, even when these are made *more* determinative (i.e., rectilinear) than they normally are. A shift in the shape of the frequency-of-calling functions is more disruptive than a shift in placement on the color continuum. What is quite striking is that a highly determinative frequency-of-calling function can be

learned much more rapidly than one in which there is a gradual transition in color naming from one color to another on the color continuum.

Now, I suspect that the difficulty in learning a set of neighboring categories with a state of equivocality prevailing in the area between the "typical instances" of each category comes precisely from the tendency to normalize in the direction of the center of one category or the other. If there is a sharp transition between one color category and another, this tendency aids learning. If the transition is gradual, it hinders it. For it is noteworthy, as in the experiment of Bruner, Postman, and Rodrigues (16) that equivocal colors are readily subject to assimilation in the direction of expected value.

It is perhaps in the realm of social perception, where the problem of validating one's categorizations is severe, that one finds the most striking effects of inappropriate category systems. What is meant here by validation is the testing of the predictions inherent in a categorization. If, on the basis of a few cues of personal appearance, for example, one categorizes another person as dishonest, it is extremely difficult in most cases to check for the other cues that one would predict might be associated with instances of this category. There is either a delay or an absence of opportunity for additional cue checking. Moreover, there is also the likelihood, since cues themselves are so equivocal in such a case, that available equivocal signs will be distorted in such a manner as to confirm the first impression. It is much as in the experiments of Asch (2) and of Haire and Grunes (33) on the formation of first impressions, where later cues encountered are cognitively transformed so as to support the first impression. The reticence of the man we categorize as dishonest is seen as "caginess;" the "honest" man's reticence

is seen as "integrity" and "good judgment."

It is perhaps because of this difficulty of infirming such categorial judgments that an inappropriate category system can be so hard to change. The slum boy who rises to the top in science can change his categories for coding the events of the physical world quite readily. He has much more difficulty in altering the socially related category system with which he codes the phenomena of the social world around him.

Inappropriate Accessibility Ordering

Perhaps the most noticeable "perceptual unreadiness" comes from interference with good probability learning by wishes and fears. I have in mind the kind of distorted expectancies that arise when the desirability or undesirability of events distorts the learning of their probability of occurrence. The experiments of Marks (60) and of Irwin (41), cited earlier, are simplified examples of the way in which desired outcomes increase estimates of their likelihood of occurrence. Certain more persistent general personality tendencies also operate in this sphere. It is indeed the case that some people are readier to expect and therefore quicker to perceive the least desirable event among an array of expected events, and others the most desired. This is quite clearly a learned adjustment to the events one is likely to encounter, even if it may be supported by temperamental characteristics. How such learning occurs, and why it is so resistant to correction by exposure to environmental events, are hardly clear. But one matter that becomes increasingly clear is that before we can know much about how appropriate and inappropriate perceptual readiness is produced, we shall have to know much more about how organisms learn the probabilistic structure of their environments. This is a point that Brunswik

has made for some years (17), and it is one that is now being taken seriously by such students of probability learning as Bush and Mosteller (18), Bruner, Goodnow, and Austin (9), Estes (23), Galanter and Gerstenhaber (25), Hake and Hyman (34), Edwards (22), and others.

There is another important feature of learning that affects perceptual readiness. It has to do with the range of alternatives for which organisms learn to be set perceptually. Put the matter this way. It is a matter of common observation that some people are characteristically tuned for a narrow range of alternatives in the situations in which they find themselves. If the environment is banal in the sense of containing only high probability events and sequences or, more properly, events and sequences that are strongly expected, then the individual will do well and perceive with a minimum of pause for close looking. But should the environment contain unexpected events, unusual sequences, then the result will be a marked slowdown in identification and categorizing. Cue search must begin again. We speak of such people as "rigid" or "stuck." George Klein's work (46) on shifting category judgments suggests that, in general, people who are not able to shift categorization under gradually changing conditions of stimulation tend also to show what he describes as "over-control" on other cognitive and motivational tasks. At the other extreme is specialization upon diversity, and how such specialization is learned is equally puzzling. I can perhaps best illustrate the phenomenon by a commonly observed pattern found in subjects in tachistoscopic experiments. There are subjects who show rather high thresholds of identification generally, and who seem to be "weighing" the stimulus in terms of a wide array of interpretive categories. Jenkin (45) has recently described such perception as "rational-

ized," the subject describing what he sees as "like a so-and-so" rather than, as in the "projective" response, reporting it "as a so-and-so." It is as if the former type of response involved a greater cue searching of stimulus inputs for a fit to a wide range of things that it "could be." It is also very likely that premature sensory gating occurs in individuals with a tendency to be set for a minimum array of alternatives, leading them into error. The topic is one that bears closer investigation. To anyone who has had much experience in observing subjects in tachistoscopic work, it seems intuitively evident that there are large and individual differences possibly worth examining here.

We come finally to the vexing problem of "perceptual defense"—the manner in which organisms utilize their perceptual readiness to ward off events that are threatening but about which there is nothing they can do. There has been foolish and some bitter ink spilled over this topic, mostly because of a misunderstanding. The notion of perceptual defense does not require a little homuncular ego, sitting behind a Judas-eye, capable of ruling out any input that is potentially disruptive—as even so able a critic as F. H. Allport (1) seems to think. Any preset filtering device can do all that is required.

Let me begin with the general proposition that failure to perceive is most often not a *lack* of perceiving but a matter of *interference* with perceiving. Whence the interference? I would propose that the interference comes from categorizations in highly accessible categories that serve to block alternative categorizations in less accessible categories. As a highly speculative suggestion, the mechanism that seems most likely to mediate such interference is probably the broadening of category acceptance limits when a high state of readiness to perceive prevails; or, in the

language of the preceding section, the range of inputs that will produce a match signal for a category increases in such a way that more accessible categories are likely to "capture" poor-fitting sensory inputs. We have already considered some evidence for increase in acceptance limits under high readiness conditions: the tendency to see a red four of clubs as either a four of diamonds or a four of clubs, with color-suit relationship rectified (14), the difficulty of spotting reversed letters imbedded in the middle of a word (69), and so on.

Let us examine some experimental evidence on the role of interference in perceptual failure. Wyatt and Campbell (86) have shown that if a subject develops a wrong hypothesis about the nature of what is being presented to him for perception at suboptimal conditions, the perception of the object in terms of its conventional identity is slowed down. This observation has been repeated in other studies as well. Postman and Bruner (68), for example, have shown that if a subject is put under pressure by the experimenter and given to believe that he is operating below standard, then he will develop premature hypotheses that interfere with correct perception of the word stimuli being presented to him. The authors refer to "perceptual recklessness" as characterizing the stressed subjects in contrast to those who operated under normal experimental conditions. It may well be, just in passing, that stress has not only the specific effect of leading to premature, interfering hypotheses but that it disrupts the normal operation of mismatch signaling systems in the nervous system. Unpublished studies from our own laboratory carried out by Bruner, Postman, and John (15) have shown the manner in which subjects misperceive low-probability contingencies in terms of higher probability categories. For example, a subject in the experi-

mental group is shown tachistoscopically a picture of a discus thrower, wound up and ready to throw. In his balancing arm and placed across the front of him is a large bass viol. A control subject is shown the same picture, the exact space filled by the bass viol now being occupied by the crouching figure of a track official with his back to the camera. The brightness, shading, and area of the viol and the official are almost identical. Subjects begin by identifying the first flash of the picture as an athlete with a shadow across him. The subjects faced with the incongruous picture then go on with reasonable hypotheses—including the hypothesis of a crouching human figure, "probably an official," as one subject put it—and in the process of running through the gamut of likely hypotheses, correct perception is interfered with. It will not surprise you if I report that the threshold for the incongruous stimulus picture is markedly higher than that for the more conventional one.

Hypotheses and states of readiness may interfere with correct perception in yet another way: by creating a shifting "noise" background that masks the cues that might be used for identifying an environmental event. At the common-sense level this can best be illustrated by reference to perceptual-motor learning where kinesthetic cues are of importance. In teaching a person how to cast a fly, it is necessary for him to guide his forward delivery by feeling the gentle pressure release that occurs when the line reaches the end of its uncurving on the backcast. If your flycasting pupil is too eager to spot this cue, he will be rather tense, and his own muscular tension will mask the gentle pressure release that he must use as a signal.

A good instance is provided by the experiment of Goodnow and Pettigrew (30) at Harvard. It is concerned with the ability of subjects to perceive a regu-

larity in a sequence of events—a very simple regularity, like the alternation left-right-left-right. . . . The experiment is done on a conventional two-armed bandit, the subject having the task of betting on whether a light will appear on the left or on the right. The task is simple. A subject is first given some pretraining, in one of four pretraining groups. One is given pretraining by learning a simple alternation pattern of payoff, another is trained to find the payoff all on one side (not easy for all subjects), a third is trained to find the pattern LLRLLR . . . , and a final group is given no pretraining. Following the pretraining and without pause, all subjects are given a series of 60 choices in which the payoff is randomly arranged, the two sides totaling out to 50:50. Immediately following this random phase, and again without pause, the payoffs now go into a stage of simple alternation, LRLR. . . . How long does it take the subject to perceive the regularity of the final temporal pattern? The speed of discovery depends, it turns out, upon the kinds of behavioral hypotheses a subject develops during the phase of random payoff. If he develops any regularity of response—like win-stay-lose-shift or win-shift-lose-stay—then he will quickly spot the new pattern. Pretraining on a constant one-side payoff or on single alternation both produce such regularity, and both forms of pretraining produce equally good results—the subject requiring but eight or nine exposures to the pattern introduced after the random phase to begin responding without error. No pretraining, or pretraining on the pattern LLRLLR . . . , does not produce the regularity of response required. Instead, the subject works on odd and constantly shifting hypotheses during the random period. When the single-alternation regularity is introduced, the result is a marked reduction in ability to spot the new pat-

tern—some subjects failing to discover the pattern in 200 trials. What we are dealing with here is interference—hypotheses and responses serve to mask the regularity of events in the environment. In order for an environmental regularity to be perceived, there has to be a certain amount of steadiness in the hypotheses being employed and in the response pattern that is controlled by it. Short of this, masking and clumsy perceptual performance results.

Now what has all this to do with "perceptual defense"? The concept was introduced some years ago by Postman and myself as a description of the phenomenon of failure to perceive and/or report material known by independent test to be regarded as inimical by the subject. It was proposed (13) that there was a hierarchy of thresholds, and that an incoming stimulus could be responded to without its reaching the level of reportable experience—as in the McGinnies (58) and Lazarus and McCleary (54) studies, where autonomic response followed presentation of a potentially traumatic stimulus without the subject's being able to give a report of the nature of the stimulus. The study of Bricker and Chapanis (6) threw further light on the concept of a hierarchy of thresholds by demonstrating that, though subjects could not report spontaneously on the identity of the shock syllables used by Lazarus and McCleary, they could guess them well in excess of chance if given a restricted choice regarding what word had been presented. I would like to propose two additional factors that might lead to a failure of perception of emotionally negative material.

It is conceivable that the estimates of probability of occurrence of disvalued events are, in some individuals, reduced—essentially the obverse of what was observed in the experiments of Marks (60) and Irwin (41), where probability

estimates were inflated by desirability. If accessibility is decreased by such disvaluation, then a cognitive counterpart of what is clinically called "repression" can be posited. It is known, however, that not everyone shows this tendency to be unready for objects and events that are anxiety-arousing. Others seem to *inflate* their estimate of the likelihood of occurrence of inimical events. Certainly one finds clinical evidence for such a pattern among anxiety neurotics. In an early paper, Postman and Bruner (68) described two types of performance with respect to known anxiety-producing stimuli, defense and vigilance, the former a heightened threshold of identification for such stimuli, the latter a lowered threshold. In a carefully designed experiment contrasting the performance of clinically diagnosed "intellectualizers" and "repressors," Lazarus, Eriksen, and Fonda (53) have shown that the former group indeed are faster in recognizing negatively charged material than they are in recognizing neutral material, while the latter show the reverse tendency. Again, I find it necessary to revert to a point made earlier. I do not think that we are going to get much further ahead in understanding hyper- and hyporeadiness for encountering anxiety-evoking stimuli short of doing studies of the learning of environmental probabilities for sequences containing noxious and beneficial events.

One additional mechanism that may be operative in lowering or generally in altering readiness to perceive material that in some way may be threatening. I hesitate to speak of it in detail, since it is of such a speculative order, and do so only because some experiments suggest themselves. It is this. Conceivably, categories for classes of objects that are pain-arousing are set up with narrow acceptance limits for stimulus inputs related to them. That is to say, what we speak of as "repression" may be the

establishment of very narrow category limits that prevent the evocation of match signals for inputs that do not fit category specifications very precisely. I am mindful that as far as autonomic reactivity is concerned potentially traumatic stimuli work in quite the reverse direction. If anything, a wide range of objects, appropriate and inappropriate, arouse autonomic reactions, without leading to verbalizable report concerning the categorial identity of the eliciting objects. Yet it is conceivable that with respect to one kind of threshold (autonomic) the acceptance limits are broad, and with respect to another (reportable awareness) very narrow. I think it would be worth while in any case to investigate the acceptance limits of inimical stimulus inputs by altering the characteristics of objects so that, in essence, one gets a generalization gradient for recognition. My guess is that the gradient will be much steeper for anxiety-arousing stimuli than for neutral ones. All that remains is to do the experiment.

Finally, it may also be the case that category accessibility reflects the instrumental relevance of the environmental events they represent. There is evidence that the recognition threshold for noxious objects about which one can do something is lower than normal, whereas for ones about which nothing instrumental can be done, the threshold is higher. That is to say, words that signal a shock that can be avoided show lowered thresholds, words signaling unavoidable shock show a threshold rise. One may well speculate whether the instrumental relevance of objects is not a controlling factor in guiding the kind of search behavior that affects category accessibility. The problem needs much more thorough investigation than it has received.

We have touched on various conditions that might lead a person to be inappropriately set for the events he must perceive easily and quickly in his en-

vironment. Many other studies could be mentioned. But the intention has not been to review the rather sprawling literature in the field, but to propose some possible mechanism affecting readiness so that research might be given a clearer theoretical direction.

CONCLUSIONS

We have been concerned in these pages with a general view of perception that depends upon the construction of a set of organized categories in terms of which stimulus inputs may be sorted, given identity, and given more elaborated, connotative meaning. Veridical perception, it has been urged, depends upon the construction of such category systems, categories built upon the inference of identity from cues or signs. Identity, in fine, represents the range of inferences about properties, uses, and consequences that can be predicted from the presence of certain criterial cues.

Perceptual readiness refers to the relative accessibility of categories to afferent stimulus inputs. The more accessible a category, the less the stimulus input required for it to be sorted in terms of the category, given a degree of match between the characteristics of the input and the specifications of the category. In rough form, there appear to be two general determinants of category accessibility. One of them is the likelihood of occurrence of events learned by the person in the course of dealing with the world of objects and events and the redundant sequences in which these are imbedded. If you will, the person builds a model of the likelihood of events, a form of probability learning only now beginning to be understood. Again in rough terms, one can think of this activity as achieving a minimization of surprise for the organism. A second determinant of accessibility is the requirements of search dictated by need states

and the need to carry out habitual enterprises such as walking, reading, or whatever it is that makes up the round of daily, habitual life.

Failure to achieve a state of perceptual readiness that matches the probability of events in one's world can be dealt with in one of two ways: either by the relearning of categories and expectancies, or by constant close inspection of events and objects. Where the latter alternative must be used, an organism is put in the position of losing his lead time for adjusting quickly and smoothly to events under varying conditions of time pressure, risk, and limited capacity. Readiness in the sense that we are using it is not a luxury, but a necessity for smooth adjustment.

The processes involved in "sorting" sensory inputs to appropriate categories involve cue utilization, varying from sensorially "open" cue searching under relative uncertainty, to selective search for confirming cues under partial certainty, to sensory "gating" and distortion when an input has been categorized beyond a certain level of certainty.

Four kinds of mechanisms are proposed to deal with known phenomena of perceptual categorizing and differential perceptual readiness: *grouping and integration*, *access ordering*, *mismatch signal utilization*, and *gating*. The psychological evidence leading one to infer such processes were examined and possible neurological analogues considered. The processes are conceived of as mediators of categorizing and its forms of connectivity, the phenomena of differential threshold levels for various environmental events, the guidance of cue search behavior, and lastly, the phenomena of sensory inhibition and "filtering."

Finally, we have considered some of the ways in which failure of perceptual readiness comes about—first, through a failure to learn appropriate categories

for sorting the environment and for following its sequences, and second, through a process of interference whereby more accessible categories with wide acceptance limits serve to mask or prevent the use of less accessible categories for the coding of stimulus inputs. The concept of "perceptual defense" may be re-examined in the light of these notions.

In conclusion, it seems appropriate to say that the ten years of the so-called New Look in perception research seem to be coming to a close with much empirical work accomplished—a great deal of it demonstrational, to be sure, but with a promise of a second ten years in which hypotheses will be more rigorously formulated and, conceivably, neural mechanisms postulated, if not discovered. The prospects are anything but discouraging.

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